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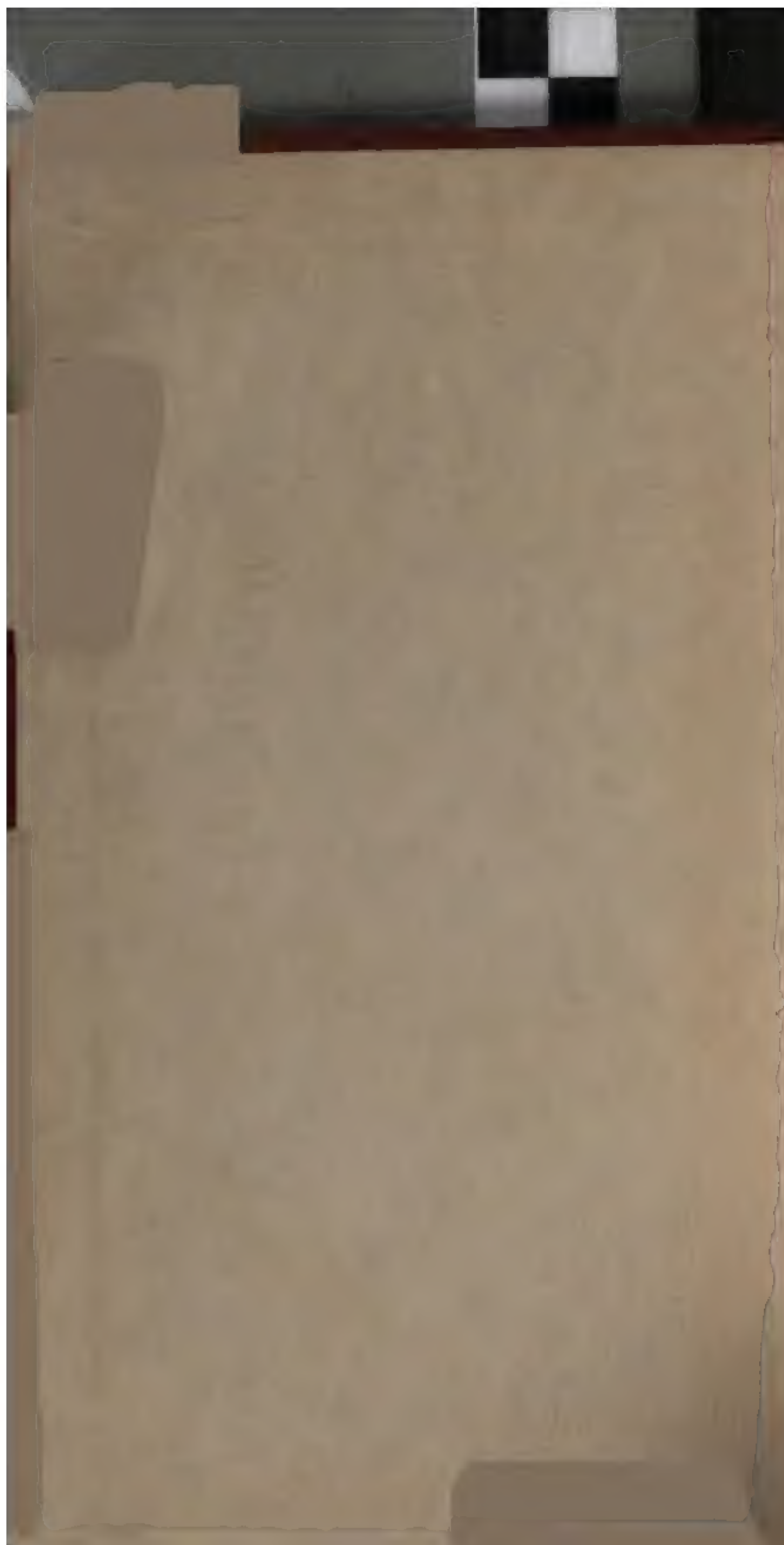
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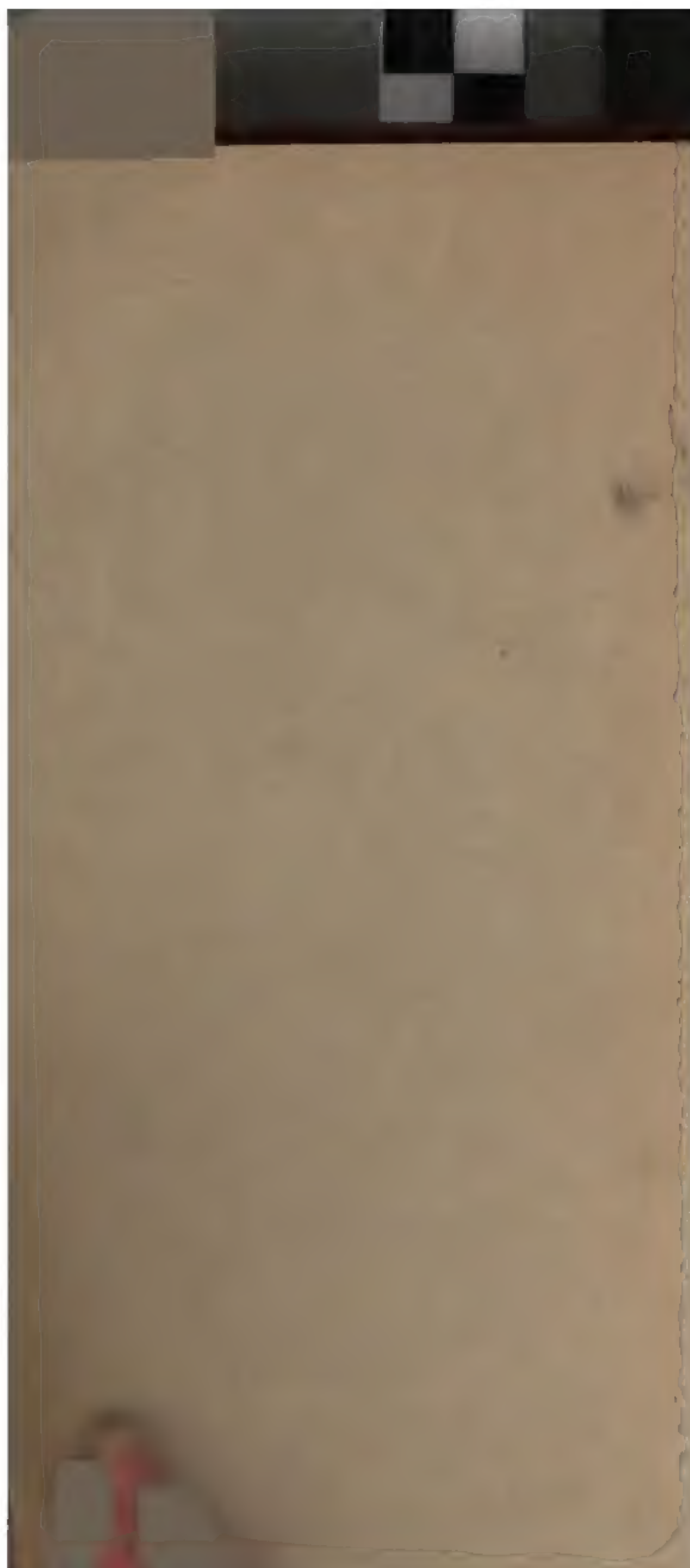


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# **ESSAYS,**

**CHIEFLY ON**

## ***CHEMICAL SUBJECTS.***

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**BY THE LATE**

**WILLIAM IRVINE, M. D. F. R. S. Ed.**

*Lecturer in Materia Medica and Chemistry in the University  
of Glasgow;*

**AND BY HIS SON,**

**WILLIAM IRVINE, M. D.**

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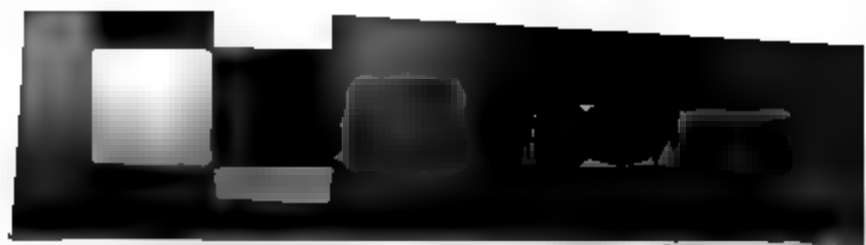
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**1805.**



THE ...  
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TO

**MATTHEW BAILLIE, M. D. F. R. S.**

THE FOLLOWING

***ESSAYS ARE INSCRIBED,***

AS

**A TESTIMONY OF THE VERY HIGH RESPECT WHICH I**

**ENTERTAIN FOR HIS TALENTS,**

AND

**THE DEEP SENSE WHICH I SHALL ALWAYS FEEL OF THE**

**FAVOURS WHICH HE HAS SO FREQUENTLY CONFERRED**

**UPON HIS MOST OBEYANT SERVANT,**

***WILLIAM IRVINE.***

*matthaei - Baillie - V. 1. 186*



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## PREFACE.

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**EIGHTEEN** years have now elapsed since the death of the author of the greater part of the following Essays, a period of time which has produced or completed one of the most signal revolutions ever experienced by the science of chemistry. That any portion of the writings of Dr. Irvine, should, under such circumstances, and after so protracted an interval, be now presented to the public inspection may be supposed to require some explanation. It perhaps cannot well be denied that whatever it is now right to publish, would have been better so done at a more early period. But this is only true with the admission that such a proceeding could have been adopted with equal or greater convenience than at any subsequent

opportunity. The manuscripts of Dr. Irvine, however, were not by any means in such a situation as to admit of immediate publication. A few essays, it is true, might have been committed to the press without much correction. But in a work of this kind the public has a right to expect, and will unquestionably demand a more ample account of the experiments and theory of Dr. Irvine than has hitherto been afforded. Such an account, however, could by no means have been compiled from any manuscripts in my possession. These, every where incomplete, would have left large intervals, the filling up of which would have exercised the patience, and have tried the knowledge of the editor. For such an office, not only a considerable acquaintance with the science of heat would have been required, but an adequate portion of enthusiasm and of interest to have smoothed the difficulties of the way. The business of an editor includes much labour, much responsibility, and little reward either in fame or emolument. If negligences appear, they are imputed to his want of care; if obscurities, interlineations, and perplexing disarrangement obstruct his way, it is his to find or to make a road through the

wilderness; of merit alone he obtains no share, or the scanty approbation of unwearied toil.

In general, no doubt, posthumous works should not be longer delayed than the indispensable attention to correctness and propriety demands. But I believe in the present case it will scarcely be denied that some further delay was necessary. For any complete account of the philosophical speculations and experiments of my father, he who might have undertaken the publication would have looked in vain amongst his manuscripts. The hiatus must therefore have been supplied by his own exertions, imperfectly assisted by a few short and indistinct references to the subject of his researches. When the difficulties are so considerable, perhaps unusual motives are necessary to stimulate the industry of the editor. Some tincture of enthusiasm, combined with a competent knowledge of the subject, might perhaps be accounted sufficient for the task. The first of these qualifications may naturally be expected in a son who treats of his father's labours. Whether I do or do not possess the last it is not for me to judge.

It has not been, however, without much

anxiety and solicitude that I have ventured to usher into public view any portion of the writings of my late father. In undertaking this duty it has been my object to explain and defend his discoveries and theories, as far at least as is consistent with candour and sound philosophy. Many of these have already found their way to light, and I believe I am not misled by that partiality so natural to my situation, when I add that they have conferred a considerable reputation upon their author. He has never, however, but from his professorial chair, had the advantage of stating his own opinions. This was undoubtedly a voluntary proceeding on his part. But he trusted for the promulgation of his doctrines to teaching them; a mode which ample experience has shewn to be at once injurious to the claims of the inventor and to the progress of science.

It appears to me, since the following Essays were not edited immediately after the death of Dr. Irvine, that the present is a favourable moment, after the publication of Dr. Black's Lectures, whose experiments and doctrines are so nearly connected with some of those contained in this work, and who was the

immediate precursor of my father in the investigation of the nature of heat. Every one of the inventors of the modern theories of heat, excepting only Dr. Irvine, has obtained, either from his own labours or from those of a posthumous editor, an opportunity of explaining and defending his own opinions. But independently of this circumstance, the theory of my father has afforded a subject of considerable discussion to the philosophical chemists. Not has it been exposed to this scrutiny without having encountered many and various objections from different authors. On the other hand, I know of no author who has taken to himself the task of examining all these objections in a fair and candid manner. Perhaps then it will not appear an extraordinary thing that such a work as the following should be undertaken, nor that the execution of it should fall to my share, connected as I am by the ties of nature to the author of the theory of which it chiefly treats.

In pursuing such a plan, I have believed it necessary to deviate considerably from my original design. I had, at first, proposed to have selected, from the manuscripts of Dr. Irvine, whatever was in a complete state regarding the subject of heat. The illustrations, the defences,



and the additions, were intended to have occupied the place of notes, which might be referred to at the end of the volume. One argument against this proceeding arose from the inconvenience of a frequent reference to long annotations, by which the train of ideas is greatly interrupted, and the subject after all is inadequately explained. But another and more powerful objection originated in the state of my father's manuscripts, none of which, excepting only a very few essays, were at all complete, or in any respect fit for publication. That part of his chemical lectures which treated upon heat, where it was reasonable to have looked for the fullest information, was totally barren, and consisted only of heads and references, in which his own peculiar opinions were not at all detailed, and even hardly mentioned. In these circumstances I adopted the plan of prefixing a few essays of my own composition, in which, to the best of my power, I should introduce the subject, endeavour to explain the doctrine of capacities, and the important inferences to which it has led, and to subjoin an answer to such objections, from theory or experiment, as I know to have been hitherto made to it. It may, at first sight, appear to

some strange and even indecorous, that I should have given the first place to the essays of my own writing: but I hope this will not be attributed to any improper feelings upon my part, but to the nature of the subject, which required that the topics explained in my essays should precede the rest.

I have, according to the views now mentioned, divided the following work into three parts. In the first of these, I have endeavoured to give an ample and accurate account of Dr. Irvine's speculations upon heat: in the second, I have placed such of his writings as appeared to me to admit of publication: in the third of these divisions, I have ventured to add a very few essays, for the matter and composition of which I am myself solely responsible.

The first part contains four essays. In these I have detailed every fact and every argument with which I am acquainted, either favouring or opposing the doctrine of capacities. If I have occasionally digressed a little from the main object of these Essays, and hazarded some observations upon parts of the theory of heat, which are not immediately connected with the subject of our inquiries, I hope for my excuse in the candour of the scientific reader, who

cannot but be aware how alluring the regions of imagination appear amidst the more solid but less attractive details of facts. In some instances I have made, what appeared to me to be needful additions; and it has been my aim, in all places, to defend the theory from those errors and inconsistencies with which it has been charged, and which, it will be readily observed, have, in my opinion, never existed, but in the minds of those who have taken a false or partial view of the subject.

It may, perhaps, be looked upon by many as a task of no considerable difficulty to collate the doctrines of a philosopher, to whose writings I have had the most unrestrained access; and this would have been perfectly just, provided these writings had been fully expressed, or had not been almost always totally silent on the subjects of which I have treated. Had any such distinct statement appeared amongst the manuscripts of my father, I should have been far from presuming to replace, by compositions of my own, any illustrations which he might have given of peculiar, dubious, or intricate points. But, unfortunately, there has been no alternative, between passing over the subject in silence, and assuming to myself the task of ex-

plaining it. I am aware that I ought to suspect myself of prejudice upon these topics ; nor am I in any respect ashamed to own my feelings of partiality. But after making every allowance for this, I still think that the theory of heat, proposed by Dr. Irvine is in its own nature so curious, as to deserve and to require a fuller explanation than it has hitherto received. This is peculiarly true with reference to its elementary points. Besides which it has been frequently, and not mildly, attacked, yet scarcely ever defended for a considerable number of years. It is not to be inferred from this that the theory has been abandoned by the chemical philosophers of the day. To demonstrate the reverse of that, I have only to mention the ingenious treatises of Professor Leslie, of Mr. Nicholson, and of Mr. Murray. But these gentlemen, contented with explaining or adopting the theory in a general way, have not entered so minutely into details, which indeed would have been inconsistent with the plans of their works; nor have they attempted to follow all the objections of preceding and contemporary writers, the insufficiency of which it has been my endeavour to point out.

There were other parts of the science of heat

besides those immediately necessary to the support of his theory, which had attracted the attention of Dr. Irvine. Wherever I have become acquainted with any observations or experiments of his upon these subjects, I have taken an opportunity of introducing a notice of them in these preliminary essays.

The second part of the following work is composed of essays written by Dr. Irvine, and which were many of them read, and all of them intended to be read, before the "Literary Society of Glasgow," between the years 1769 and 1784. Each essay is marked by its peculiar date wherever I have been able to ascertain that point. Since the period at which they were composed, the changes in the science of chemistry have been immense, and altogether astonishing. That branch of knowledge has advanced with gigantic strides, and has reared its head to a height which the boldest speculator could not have foreseen or expected. In these Essays, however, I hope little will be found inconsistent with the present state of chemistry. I have, in general, where alterations have taken place, marked them in the margin; at least, where they appeared to be of sufficient importance, or at all likely



mislead. It ought not to be expected, however, that essays brought forward under the present circumstances should contain, or be made to contain the more recent improvements. In selecting them from the manuscripts of Dr. Irvine, I have been guided by different principles. It appeared to me, wherever I could find any complete essay regarding heat, in which peculiar doctrines were maintained, that such an essay would be acceptable to the public, though they may already be in possession of the principal facts contained in it. There is always a desire to understand the terms and method employed by any discoverer or inventor of theories.

I have also added to the collection all the tracts in which any new principles or facts are announced, of which the public are not in possession. In general I have offered to the world whatever I have found amongst the manuscripts of Dr. Irvine which appeared to my conception to contain novelty, or to be treated with peculiar ingenuity or propriety. In these procedures I have been guided almost intirely by my own judgment and taste. The candour of the public, therefore, will not fail to direct that censure which it may think pro-

per to adjudge into its proper channel, if in this respect there shall appear just cause for censure. It will not be difficult for an accurate observer to perceive that none of these Essays were even intended for the press. It would be unfair, therefore, to attribute to their author any other inaccuracies than those of reasoning; and even of them, where they exist, it cannot be doubted that the hand of the original composer could have extinguished or abridged the number. Where I have been able, I have added the corrections which appeared to me needful. But I am reluctant that those blemishes which may have escaped my diligence or eluded my observation should occasion censure to any other than their editor.

Upon the whole, however, the very design of publication shews, on my part, a conviction of a different result, and I am persuaded that the public will not receive without some degree of interest these specimens of the method of reasoning and experimenting employed by Dr. Irvine. The pursuits of an ingenious mind cannot but afford amusement as well as instruction, and I am impelled to bestow some confidence on the remark of the late learned Dr. Robinson, combined as it is with



most unmerited compliment to myself, "that the studies of such a man as Dr. Irvine must have been extremely ingenious and important." I will now conclude my prefatory remarks on the second part of this work, and only further observe that, if I do not deceive myself, there will be occasionally found in these Essays, remarks and views which even now are not familiar to the cultivators of philosophy.

There is another essay of Dr. Irvine on nervous diseases which has always appeared to me to contain some novelty and ingenuity of conception, and to which I should, perhaps, have given a place in this work, but that I have already made it the ground-work of an inaugural dissertation, "*de Epispasticis*," which I published at Edinburgh in the year 1798.

With regard to the third part of the work, I have little to observe. As it is by my voluntary deed presented to the public, it must await and submit to their judgment. The essay on latent heat was formerly printed in Nicholson's Journal, though in a more imperfect and abbreviated form.

The substance called formerly black man-

ganese, and now black oxyde of manganese, has been long known and employed in the arts. The discovery of the metallic nature of this substance has been hitherto chiefly attributed to the Swedish chemists. What I am now about to advance cannot be regarded as an attempt to deprive these philosophers of any part of their well-earned merit. But since I do not entertain any doubt of the same discovery having been made in Britain at rather an earlier period than in Sweden, it would be unjust to omit the mention of it. The difference has been that in the latter country it has been committed to the press; while in the former, another mode of communication, that of teaching it, was adopted, not equally successful in disseminating the knowledge of the facts.

Amongst the notes of experiments performed by Dr. Irvine, which are in my possession, there appear a very great number on the black oxyde of manganese. In these he formed almost every combination of which that body is susceptible, and even produced and remarked some of the peculiarities of oximuriatic acid gas, though he was unable to condense it, having attempted to receive the

product in a receiver without water. Of consequence the extraordinary properties of that substance were not detected by him. In referring to the article manganese, in the notes which he employed for his chemical lectures, I find the following passage, which contains the greater part of his observations upon this subject.

“ In the year 1769 I was engaged in experiments on the colours used in painting on enamel. Among other substances manganese was examined; and as in some cases the colour which it communicated to glass resembled that which is given by gold, I hoped I should be able to prepare it in such a manner as to make the colour still more beautiful. I accordingly subjected it to many operations, and particularly digested it with all the mineral acids, and from the phenomena which I observed in mixtures of manganese and acids, as well as from the colour, I concluded it was a metallic substance; but whether a new metal, or a mixture of those already known, I could not at first determine. Upon examining the solutions in acids more carefully, I concluded that it contained a metal of a peculiar kind, and this is evident from the following effects of acids

upon it. It ought to be observed, however, that besides a new metal, manganese often contains some old metal. The manganese which I employed in all my experiments is that used by the potters and at the glass-houses, commonly called by the French *manganese de verriers*. It is found at Mendip hills.

“Manganese reduced to powder and exposed to heat retains its black colour, and if the heat is raised to bright red, the powder coheres, so that it cannot be separated from the vessels. The alteration is inconsiderable, unless the manganese was mixed with a large quantity of calcareous spar, which is not commonly the case. I generally picked out this spar as well as possible before I exposed the mass to heat. In a violent fire it runs into a dark-coloured slag, which, however, is never in complete fusion. It is usual to calcine the manganese before it is used in painting, which is perhaps of no great importance.

“Manganese, mixed with the different *soffs* acids, is more or less dissolved in them. I dissolves with most violence in common *smol*ing nitrous acid, more completely in *muria* acid, but soonest and best in aqua regia composed of two parts of nitrous acid, and

of muriatic acid, in which it may be completely dissolved. The whole does not admit of solution in nitrous acid; but the part which is undissolved having all the properties of manganese, I imagined that this was owing to the want of phlogiston. Accordingly, on heating it red-hot with charcoal dust, and then adding nitrous acid, it dissolved completely with violent effervescence. This first convinced me that it was a metal of a particular kind. By digesting manganese in spirit of wine, it may be all dissolved in nitrous acid, and this may be done even by exposing the manganese and acid to the sun's rays. By these means manganese may be completely dissolved in all the acids, and by my experiments there never remained  $\frac{1}{200}$  part, and in some cases nothing. The best way of getting a pure solution of manganese is, first to calcine it, then to infuse in the nitrous acid, which will take up the calcareous spar, &c. mixed with it, then to wash it well, dry it, and phlogisticate the remainder in some of the ways mentioned above. It may then be dissolved in the acid.

“ Fixed alkali precipitates manganese from acids in form of a white powder, which, laid



on a red-hot iron, burns and becomes black. Treated in a phial as in the operation for Homberg's pyrophorus, it forms something like it, but it requires more heat to set it on fire.\*

Amongst the other notes of these experiments upon manganese, I observed an allusion to an experiment of the celebrated Mr. Watt of Birmingham, in which he had been able to procure a few metallic globules from manganese by heating it in contact with combustible substances. I was induced by this to write a letter to Mr. Watt, requesting of him to inform me whether he had made such an experiment,

\* In another place I find the following passage. "An ounce of manganese and two drachms of crude tartar well mixed in fine powder, in a covered luted crucible, were made red hot for fifteen minutes. The upper part was brown, the under green, and the middle purple mixed with dirty green. It had concreted a little, but crumbled between the fingers. When not hotter than boiling water, it set fire to paper like Homberg's pyrophorus." This experiment has not given the same result in my hands. A more expert operator might be more successful. Perhaps this combination, probably one of charcoal and manganese, may throw some light on the varieties of metallic manganese, some of which moulder rapidly into a black powder on exposure to air, others do not. It must be remarked that all the experiments from which Dr. Irvine inferred the metallic nature of manganese, are not related above; indeed only a very small part,

and if he was acquainted with any speculations of Dr. Irvine upon this subject. In return to which I was favoured with an answer containing the following passage, fully confirming, by the testimony of a living and most highly respectable evidence, the truth of what has been above asserted.

“ I remember perfectly the experiment you mention, about the year 1769. The globules were found principally attached to the cover of the crucible, and were afterwards discovered to be lead alloyed with manganese, as they stained glass purple. Seeing these metallic globules, suggested to your father the trying of the manganese in the way of solution, when he found its solutions had the properties of those of a metal. And these facts took place long before we heard of the Swedish experiments on that metal. We, however, were never able to reduce it to a regulus, at least that I know of.”

Since the above communication from Mr. Watt, I have learnt from some other gentlemen that the experiments of my father were well known at that period, and that he was pressed by his friends to offer them to the public, which however, he unfortunately neglected to do in



any other way than by detailing them in his lectures. I may mention the name of Dr. Patrick Wilson, late professor of astronomy in the university of Glasgow, as one of those gentlemen from whom I received this information.

From the statement now given it appears that Dr. Irvine and Mr. Watt have joint claims on the discovery of the existence of a peculiar metal in black manganese. Their experiments were fully more early than those of the Swedish philosophers Bergman, Scheele, and Gahn. The investigation was undertaken in both countries, in all probability, without the slightest knowledge of similar attempts being made in another place.

I have introduced the mention of these facts into the preface, because I did not perceive any opportunity of inserting them with advantage in the body of the work.

I may notice here, that the freezing of the water in the calorimeter observed by Mr. Wedgwood, as remarked in Essay IV. Part I. page 130. was also found to occur by Mr. Accum, as I am informed by himself, in experiments made with the calorimeter in the laboratory of the Royal Institution. This

information I did not receive in sufficient time for insertion in its proper place.

It is also proper to remark, that M. de Luc has justified himself from the charges preferred against him by Dr. Robinson, in a letter addressed by him to the writers of the Edinburgh Review.

In the first essay I have made some very brief observations on the question of the necessity of the particles of matter not touching each other, in which I have endeavoured to shew that a partial contact might be made to agree with the phenomena. Since writing these remarks, I observe in Nicholson's Journal, that a French author has entertained the same opinion, with which, however, I was intirely unacquainted at the time of writing my remarks,

Since the first part of the following Essays was printed, I have met with some observations of M. Berthollet in his Chemical Statics, on the subject of heat. He there discusses the merit of the theory considered in the following work. After noticing the doubtful arguments which have been deduced by M. M. Lavoisier and Laplace, from the wide variations of the natural zeros, calculated according to their

experiments in the calorimeter, he adds, that they found the capacity of a solution of one part of nitre in eight parts of water less than should have been indicated by the expression of the specific heat of the water of the solution alone, which would have been .88889, whereas by experiment it was only .81670. Cold is produced in the formation of this solution, and the capacity, instead of being less, ought, according to the theory, to have been greater. This argument, if perfectly established, would certainly, as I have observed in the following Essays, be decisive. I need not here, however, repeat the very strong objections which can be proposed against the accuracy of the calorimeter, on which, in this case, every thing depends. But I resolved to repeat the experiments on the capacity of the solution in question with as much accuracy as possible in the way of mixture.

The medium of comparison which I used was pounded glass. The capacity of various glasses being very different, I first found by experiment the glass used to have a capacity of .190. I then made three experiments, mixing this glass with the solution. Each time I used five ounces of each of the materials at

different temperatures, and the results approached each other as nearly as is to be expected. The mean deduced capacity was .914. The temperature of the glass was never higher than  $120^{\circ}$ , nor that of the solution lower than  $44^{\circ}$ .

In offering my experiments, in opposition to those of men so illustrious as the authors of the *Memoir upon Heat*, I am not insensible of the risk of the accusation of temerity. Nor is it without feelings of the most unfeigned diffidence, that I venture to oppose the opinions of philosophers of so high a character. My excuse must be "*amicus Plato, amicus Socrates, sed magis amica veritas.*"

In the Essay I. Part I. I have mentioned an argument illustrating the possible decomposition of water in the galvanic trough, and the appearance of the hydrogen and oxygen at a distance from the spot where they must have been produced. I allude to the similar occurrence in the decomposition of the acetite of lead by zinc. It is proper to state that I learned this argument in a conversation with Mr. Nicholson, though I am uncertain whether he claims the invention of it,



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**PART THE FIRST.**

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**ESSAYS**

**ILLUSTRATIVE AND EXPLANATORY**

**OF**

**DR. IRVINE'S THEORY OF HEAT.**

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**BY WILLIAM IRVINE, M. D.**

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**"Veteris vestigia flammæ."**



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# ESSAY I.

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## ON THE NATURE OF HEAT.

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**T**HE idea of heat is perfectly familiar and simple: In common language every person understands by that word a peculiar sensation excited in the human body by the approach or contact of hot substances. In the philosophical acceptation, the phrase heat is used to express the cause of this sensation, whatever that may be.

Heat has been the subject of many inquiries, and a number of its properties has been successfully investigated. From its universal diffusion and its presence or interference in every chemical process of nature or of art, it has attracted an unusual portion of the attention of chemical philosophers. Nor can it be said to be unworthy of the investigation which it has received, since in theory it forms one of the

most curious pursuits, and in practice one of the most useful and most universally applicable to the purposes of life of any of the objects of scientific research.

In the new nomenclature the word caloric has been adopted to express the cause of the sensation of heat, and therefore bears exactly the same meaning which the word heat has always done in scientific works since the attention of philosophers has been directed to this subject. There is surely now little or no danger of ambiguity from the confusion of the cause with the effect of heat, whatever may have been formerly the case. I am therefore inclined to consider these two words as possessing almost equal merits, and to be so nearly balanced in the scale of excellence, that I have indulged myself in the indiscriminate use of both, guided by no other motives than those of convenience. In this procedure I think I am justified by the practice of the best and late chemical writers of this country. Verbal criticism is not often useful in science. If either of the words in question be clearly defined, we shall be at no loss to understand the subject of our arguments by whatever name it may be called. Besides the philosophy of heat has received many, I believe I may say most of its improvements in this island; and in the writings of the British chemists the word heat has been always at least partially employed. For these reasons, to say nothing of the circumstance that the propriety of the phrase caloric itself

has been questioned, I shall use in the following pages indiscriminately both these modes of expression.\*

Heat or caloric is universally diffused: No substance has been observed to exist totally deprived of it. It may be partially withdrawn; a body may be rendered greatly colder than it ever appears naturally at least in these latitudes; but there is not the most remote reason to imagine that a body in such a situation does not still contain a large supply of caloric.

Till very lately it was not supposed that heat ever existed separately from one or other of the various species of matter composing this earth, or from the rays of light. Perhaps it may even yet be asserted, that no philosopher has been able indisputably to produce a quantity of caloric without employing as a vehicle for it some substantial body. Hence it has been difficult to advance positive proof that heat is matter.

The curiosity of philosophers, however, has not been restrained by such embarrassments. Many opi-

\* The whole quantity of heat in any body was usually named by Dr. Irvine its absolute heat, in contradistinction to relative heat; and when the whole caloric contained in any substance is considered, this expression is peculiarly clear and convenient, and has accordingly been adopted by most writers on this subject. The absolute heat or caloric of any body therefore means the whole quantity of the cause of heat residing in that body, in whatever state or combination that may be.

nions have been proposed concerning the nature of caloric. Some of these have regarded it as a compound : others, on the contrary, as a simple substance. Whilst a number of philosophers have refused altogether to acknowledge its material nature, and are disposed to account for the phenomena of heat by supposing a certain tremulous or vibratory motion among the ultimate particles of which bodies are composed.

The illustrious Scheele conceived that caloric was compounded of oxygen and phlogiston, and that light was a compound of the same nature with different proportions of the ingredients. This theory accounted very well for a great number of facts, and was not unworthy of its celebrated author. It is now, however, only mentioned in the history of opinions once held upon this subject, since the foundation of the whole superstructure has been removed by the revolution which has expunged the name of phlogiston from the list of chemical agents.

A much more recent hypothesis has represented caloric as compounded of positive and negative electricity. The phenomena of the galvanic trough have given rise to this opinion originally proposed as far as I have learned by Dr. Gibbs of Bath. It is a well known fact, that while the elicitation of positive and negative electricity is going on from the different sides of the pile or trough, a small stream of oxygen and hydrogen gases is discharged, the



oxygen gas from the side which produces the positive electricity, and the hydrogen gas from that which produces the negative. From these circumstances it is inferred that it is the combination of water with the positive electricity which forms the oxygenous gas, and the union of water with the negative electricity which forms the hydrogenous gas. When therefore these two gases are forced to combine, their water must be deposited, and the positive and negative electricity unite in the form of caloric. Thus far this hypothesis gives a very clear and ready account of the facts. Before admitting it, however, it must undergo a more severe investigation.

One of the circumstances which has given origin to Dr. Gibbs' theory has been the decomposition of water in the galvanic trough, the two component or supposed component parts being given out at a considerable distance from each other. Richter imagined that this fact sufficiently proved the falsehood of the theory of the composition of water first proposed by Mr. Cavendish and Mr. Watt, and commonly called the Lavoisierian theory. He could not conceive that the same particle of water could be influenced at two different places at the same time. Now this is certainly a considerable difficulty, and it may not be easy altogether to remove it in the present state of knowledge of the action of the particles of matter on each other. But it can be readily shewn that similar embarrassments oppose

the explanation of various other chemical phenomena. If, for example, a piece of zinc be hung in a solution of acetite of lead, the lead is separated in the metallic state, and forms a very beautiful tree. At the beginning of this process a particle of zinc meets with one of acetite of lead, which it decomposes, and the lead is deposited at the point of action. This happens, however, only at the beginning; for as the decomposition goes on, the lead is deposited farther and farther off, and near the end of the operation at a very considerable distance from the zinc. This is undoubtedly a very curious and difficultly explicable phenomenon, and resembles the case of the decomposition of water by galvanism in the main point, to wit, the deposition of the parts of the compound at a distance from each other. Yet this, as far as I know, has not been hitherto supposed a sufficient ground for denying that acetite of lead is formed by the union of acetic acid and oxide of lead.

Besides this separation of the parts of a compound is not by any means a very rare occurrence. It may be observed in all precipitations of metals in the metallic state. The arbor Dianæ shews phenomena extremely similar. If attention be paid to the formation of crystals in any saturated solution, of muriate of soda, for example's sake, it is, I believe, admitted that if evaporation be prevented, no crystals will be formed. But if evaporation be permitted to proceed, the salt ought to be deposited at

the surface, because each particle of the compound of salt and water is there decomposed. But it is a certain fact that the deposition very often takes place at the bottom of the vessel. So that here a decomposition is effected at a given point, and one of the ingredients makes its first separate appearance at a considerable distance from that point.

These examples occur to me as sufficient to shew that, supposing water to be compounded of oxygen and hydrogen gases, the production of these gases in the galvanic trough at a distance from each other is not an unique phenomenon in chemistry. Whence it should follow, that it is not necessary to invent a peculiar theory to explain it, or at least that this theory should apply in the other cases as well as in the present. Now, though it may be possible, and is not by any means unlikely, that electricity may interfere in many chemical processes where its agency is not now suspected, it does not appear how Dr. Gibbs' hypothesis can apply to the decomposition of acetite of lead or saturated solutions of various salts.

Besides, as has been well observed, water of ammonia being exposed to the galvanic action, hydrogen and nitrogen are produced, and no oxygen at all, which cannot be reconciled with the principles in question. It is also extremely doubtful whether the two kinds of electricity can be considered as distinct substances, which ought to be ascertained before so important deductions be made from it. Upon the whole, therefore, it appears that though we can ob-

serve much ingenuity in this theory of Dr. Gibbs, there are not yet brought forward sufficiently strong proofs to convince us of its truth.

Of the other hypotheses of the nature of caloric one considers it as a substance of extreme subtilty, penetrating with ease even the densest and hardest bodies, contained in considerable quantities in all matter, and the particles of which repel each other the more strongly the nearer they are forced to approach. This opinion receives some degree of illustration, if not of support, from the analogy of the electrical fluid. Between caloric and electricity, indeed, there seems to be a considerable but obscure analogy. Neither of them has hitherto been proved to be material, by the test at least of gravitation. Electricity can be substituted for heat in giving origin to combustion and in fusion, and resembles it in the property common to both of being accumulated by friction. This analogy, however, is very far from being complete. Fusion by electricity is certainly materially different from that by caloric, since a metal may be melted by electricity while the cloth in which it was enveloped remains untouched. It probably consists in a momentary separation of particles from the repulsion supposed to be communicated to them by the electric power. Caloric in many instances may be supposed to fuse in a similar manner, at least in those cases where the solid is dilated during its passage into the fluid state. It has been observed, however, by Van Diemen, that metals, after



fusion by electricity, are left with a loose powdery appearance. The cause of this may be, first, that the electricity not being repelled by particles similar to itself, passes off with greater rapidity than caloric can do, which meets caloric on every side: secondly, that the capacity of the fluid for caloric is enlarged, or in other words, the fluid contains much latent heat, which it must lose before becoming solid, and therefore a considerable space intervening before the solidification is finished, the particles have time to arrange themselves, as directed by their mutual attraction for each other. Probably then the capacity of a body for electricity is not much increased by its fusion by the electric matter, unless the want of electric matter in the surrounding bodies be thought sufficient to account for the rapid dispersion of that for a moment accumulated in the fused body.

All solids become hotter by being rubbed; a certain class only accumulate electricity. Some bodies do not transmit the electric fluid at all, or with extreme difficulty; all bodies transmit heat, and though there are different conducting powers of different substances for caloric, yet this difference is by no means to be compared with the reluctance of the non-electrics to transmit electricity. It is possible that many of these inconsistencies may disappear before a more accurate and extensive knowledge of heat and electricity.

The existence of heat, as a peculiar substance, is very hypothetical. It must therefore fall to the lot

of those who defend that opinion, to shew that such a supposition will account for all the phenomena, and that it is impossible to explain them without it. If they fail in either of these, their argument cannot be just. If caloric be matter, it at least cannot be shewn to gravitate. Many experiments have been made to determine this point. At one time these were supposed to prove that caloric really did gravitate. Afterwards it was imagined that more accurate trials had proved that it not only did not gravitate, but actually lessened the former weight of the body containing it, and was a principle of levity. And very ingenious theories were devised to explain this curious phenomenon. It seems now agreed, however, from the latest and best experiments, that caloric possesses neither weight nor levity, and does not affect the gravity of the body into which it enters.

Caloric passes, though very slowly, through the Torricellian vacuum. This has been considered as a proof of its material nature, since a quality or species of motion of a vibratory or other kind cannot, by any ingenuity of hypothesis, be supposed to penetrate vacuity. And perhaps this argument might be conclusive enough, provided it could be shewn that the Torricellian vacuum is really a portion of space unoccupied by matter. But it can only be shewn to be a vacuum of air, and may, nay certainly does, contain light, electricity, and perhaps other invisible, and yet unknown substances.

The strongest arguments for the existence of caloric as a distinct species of matter are derived from the observation of what has been called radiant caloric, and more especially from the ingenious and curious experiments of Dr. Herschell, on the separation of heat from those of light. This gentleman has found, that the prism divides the solar beams not only into different coloured rays of light, as has been long known, but also into invisible rays of heat, which are governed by peculiar laws in their refractions and reflections. This discovery, both unexpected and curious, has served to convince some philosophers that beyond all doubt caloric is proved to be a peculiar and distinct species of matter, separable and now separated from all other substances. It is not, however, altogether finally decisive of this question, and it may perhaps be worth while to consider, how far the probability of the material nature of caloric is increased by these experiments.

It was on the side of the spectrum next the red ray that the rays of caloric were found. On the opposite side next the violet ray certain other rays have also been discovered, which do not produce heat, but which have a greater power of reducing metallic oxydes than the rays of the ancient spectrum. These rays, as well as those of caloric are wholly invisible, and their existence is inferred only from some chemical properties which they appear to possess.

Mr Leslie, in a very ingenious work upon Heat, has lately turned his attention to this subject. He



has with great industry and sagacity performed many curious and valuable experiments, the results of which are far from coinciding with those of Dr. Herschell. He has certainly shewn that the radiation and absorption of heat are guided by laws essentially different from any before imagined, and the facts which he has discovered are in themselves highly interesting, independent of all deductions from them. But his object has been to disprove the existence of radiant caloric, and to shew that while heat and light are essentially the same, heat is not at all conveyed from hot bodies to colder ones in rays, but that the air is the vehicle of conveyance, and is affected by pulsations from the sudden rarefaction of the portion next the hot substance, in a way not altogether dissimilar to that in which the same medium is influenced by the vibrations of any sonorous body. It cannot be denied that he has given considerable probability to his assertions, and that his experiments and theory appear to agree in many points. Yet the subject is by no means completely elucidated. Mr. Leslie has partly endeavoured to invalidate, partly to doubt Dr. Herschell's experiments. In his attempts to effect the last of these, every one will not be disposed to join, and Dr. Herschell's investigations must receive too much support from his known ingenuity in other branches of physics, to be combated in any other way than by experiments directly contradicting those before the public. Mr. Leslie, however, has shewn some cause to hesitate,

before the proposed inferences be admitted. There are other rays besides those directly from the sun, which pass through the prism, and are to be found beyond the red ray. Something should be allowed for reflection, and something, as Mr. Leslie thinks, for inaccuracy. The effect of a stream of air, in arresting the progress of radiant caloric, is also very remarkable, and much against Dr. Herschell's hypothesis.

The notion of invisible rays of heat has been long entertained: the novelty of Dr. Herschell's theory consisted in placing them among the solar beams, as separate from the rays of light. It is clearly supposed that the invisible and heating rays are pure caloric. If it could be imagined that they were any modification of light, we should be disposed to account for the appearance of heat in the old way, at least no proof of the contrary doctrine would have been adduced. Dr. Hutton and other philosophers have doubted as to the necessity of visibility to the existence of light, and, as it seems to me, on very good grounds. It is extremely probable, that either the air or the combustible substance contains light invisibly, which the process of combustion afterwards develops. The compositions called pyrophori can be deprived of their power of emitting rays of light by exposing them to severe cold. This luminous quality can however be restored to them, by a very moderate rise of temperature. In this case, if light and heat be really different, it can scarcely be doubted

that the light existed in the frozen pyrophorus in an invisible state. Hence, it is only light in a peculiar state that can be seen. A certain intensity of light, moreover, is necessary for exciting vision. It is well known that some animals can see distinctly where the human eye has no perception. The human eye itself varies in its power in different persons, and even in the same person, under different circumstances. Visibility therefore is not indispensable to the existence of light, but only to a peculiar mode of its existence. Light we certainly know to be susceptible of numerous modifications, as manifestly appears from the various coloured rays of which the solar beam is composed. These rays differ eminently in their powers of heating, no less than in their other properties; and their phenomena suggest the well-grounded suspicion that the chemical affections of light are yet but imperfectly understood. Light is transmissible through certain bodies, called from that property transparent. But radiant caloric is not transmitted through all these substances. Some, such as glass, refuse a passage till they are themselves saturated with it. Even the air, which transmits caloric from fires and other chemical combinations, is itself affected by the heat, as Mr. Leslie's experiments clearly shew. But the solar rays of radiant caloric pervade air like the rays of light themselves, leaving no trace of their progress. One cannot admit the alleged facts as positive proof of the material nature of caloric, after such reflections. If light and heat

be indeed separate bodies, perhaps their mutual combinations may serve to alter and disguise each in a sufficient variety of ways, to account for every modification undergone by either.

The caloric can scarcely be all radiated from a cooling body, nor even the greater part of it : since in that case hot bodies would cool fastest in the rarest media, or in a vacuum, and in air sooner than in iron filings or in mercury. But the very reverse is the fact. Hence there must be a power of attraction in the receiving body, as well as one of repulsion in that emitting caloric. The cold reflected by mirrors in Pictet's and Leslie's experiments is referable to this head, and is certainly a most curious phenomenon. Dr. Hutton accounted for this by supposing that the temperature of all bodies is kept up by the radiation from surrounding objects, and that of consequence the vicinity of cold bodies, which radiate less heat, would be equivalent to the subtraction of heat from the thermometer used. And this certainly does explain the depression of temperature, in as far as it may be occasioned by the mere approximation of cold bodies ; but by no means, why this depression should increase, when by the reflecting mirror more radiant heat is thrown on the thermometer. One would think that the temperature ought to be raised by such a direction of the rays of heat. For though the mirror concentrates into one focus, upon the bulb of the thermometer, many rays from the surrounding bodies, which rays would



not otherwise strike that instrument, the thermometer does by no means make an equal return, but at most can only radiate a few rays to the mirror, proportioned to the minuteness of its mass, and the nature of its surface, now proved to have little disposition for plentiful evolution of caloric. In fact, it seems necessary to allow, that some positive cold is directed by the mirror, and I feel myself disposed in these circumstances to listen with a favourable ear to the ingenious account lately proposed of this, and similar phenomena, by Mr. Leslie and Count Rumford. Mr. Leslie's experiment, in which he found that agitation of the air, through which heat is radiating, materially affects the passage of the supposed rays, is indeed very stumbling to the adherents of the opposite theory. At the same time the passage of heat through the Torricellian vacuum is a little in the way of Mr. Leslie's theory, unless we grant that the supposed vacuum is a plenum occupied by vaporised mercury, which may be supposed to perform the office of air, in the conducting of heat. There is an experiment easily made, which may be used to illustrate the conveyance of heat suddenly to some distance by heated air. If any hot piece of iron or steel, (the point of a large pair of common snuffers answers very well) be covered with liquid tallow, in which there float any small black substances to shew the motion of the fluid matter, and the iron be then suddenly brought in contact with the flame of a candle, the portion of melted tallow next the candle is imme-

diately thrown with some violence through the colder tallow, probably nearly as the heated air is repelled from the hot substance, according to Mr. Leslie's hypothesis. Another curious circumstance may be remarked in the same experiment now related. If the point of the steel instrument be retained for some time in contact with the flame, the whole of the fluid is repelled to some distance, leaving the hotter part of the iron perfectly uncovered.

Besides these considerations which must induce us to hesitate, before we admit as proved the discovery of the solar calorific rays, there is another fact which seems to me to increase the doubt upon this subject. Messrs. Wollaston, Ritter, and Bockman have demonstrated, that on the opposite side of the spectrum, beyond its verge, certain substances, as muriate of silver, placed there, suffer a more rapid de-oxydation than in any other situation. From whence it is inferred, apparently with justice, that there must exist in this space some influence connected with the solar rays, and this they conceive to arise from some other invisible rays accompanying the common rays, but differing from them in refraction. That these rays are not rays of caloric we certainly know. But they are sufficient to prove that all the solar rays are not visible, and that the quality of invisibility does not afford sufficient grounds for denying the presence of rays which are not heat, and which may therefore exist in the calorific rays in dispute.

It has also been observed that a thermometer of

which the bulb is blackened shews a greater increase of temperature when the calorific rays are directed to it, than a thermometer with a shining bulb does in the same situation. In this circumstance there appears much analogy with light, and but little with heat. Black bodies no doubt, and dark coloured bodies in general, have a greater power of absorbing light. But as far as I know, it has never yet been asserted that caloric shews any preference to particular colours. And if it shall appear by further experiments that the radiant heat follows the same laws in absorption as light does, we shall have still further reason no doubt whether the calorific rays are pure heat.

I wish it to be observed that I am far from objecting to the truth, or undervaluing the merit of Dr. Herschell's experiments. I have only endeavoured to question the certainty of an induction from these experiments, the truth or falsehood of which, however, does not necessarily affect that of the experiments themselves. One may be induced then, for the reasons which have been assigned, to remain in doubt concerning the materiality of caloric, and especially of the justness of the recent arguments adduced to prove it. It may be allowable to assert that there are at least three varieties of the solar ray: that the central rays of the spectrum contain visible light and heat; that the calorific rays contain heat, but not visible light; and that the last discovered kind contain neither of these. It may be asserted



that there are altogether nine modifications of the sun's beams, each, certainly, in some respect differing from the other. And so far the process has not departed from the road of fair induction. But when we advance beyond these limits, and affirm that the calorific rays are pure caloric, it seems to me that we presume too much on that slender knowledge of caloric, which even the successful labours of some of the ablest men of the last hundred years has procured, of that most difficult, most curious, and yet most obscure subject. Upon the whole, the hypothesis of the materiality of heat, which can be supported by many ingenious arguments, and which is highly convenient for the explication of various chemical phenomena, yet seems to want somewhat of the clearness of complete proof.

Another theory has represented caloric as wholly immaterial, and as consisting of certain vibratory motions among the particles of matter. This notion has probably taken its origin from the observation of a very common fact, that all solid bodies produce or collect heat, when subjected to friction. In some cases this production of heat is accompanied by a change in the substance producing it, as when iron is hammered, as it may easily be, to incandescence, which can only be done once. In other cases this does not happen, as in the case of two pieces of wood, which will become hot as often as they are violently rubbed against each other. These and similar facts naturally enough suggested this theory, that caloric

is not matter, but motion, an opinion which can rank among its supporters some of the highest names in modern philosophy. It has been found, however, extremely difficult to express clearly the nature of this motion. It certainly does not exist in every instance in which the particles of bodies move upon each other. It is not every motion, but a motion *sui generis*, or one different from the ordinary motions of particles. When bodies uniting chemically together have their temperature diminished, their particles certainly move, while the very reverse of heat is produced. Fluids, however, much agitated, do not become hot. Yet it is not easy to conceive how fluids can be so heated without internal motion of the integrant particles. Nor does it appear that any heat can be produced by agitating gaseous bodies. In these two last instances it has been supposed that the non-production of caloric is owing to the circumstance that there is too little friction. Friction is on all hands admitted to excite heat, but the way in which it does this is not so clear; and the impossibility of producing a rise of temperature by moving the particles of fluids is a considerable difficulty in this explanation of the nature of caloric. Whether the particles of matter nowhere touch, but are sustained at a certain distance from each other by the contrary powers of attraction and repulsion, it is difficult to decide; and still more abstruse is the question, whether particles in such a situation, mutually change their relative situations, or perform any revo-

lutions around a common or peculiar centre. When it is considered that it has puzzled the ablest mathematicians of Europe to determine the motion of three particles of matter, what must the difficulties of an inquiry be which extends to a series of these minute bodies, so much more numerous and perplexed? Yet in the prospect of the possibility of any such motions existing, it seems impossible to decide how they may be modified or combined to produce the phenomena of caloric. If philosophy should ever be enabled to illuminate the darkness of so obscure a subject, it is perhaps not less likely that the motions of the particles of matter amongst each other should be employed to illustrate the nature of electricity and light, than that of heat. The chief advantages of a theory which supposes caloric to be motion are, that it does not necessarily imply the materiality of heat, and that it appears readily to account for the increase of temperature produced by friction. The phenomena of latent heat have been supposed to be irreconcilable to this hypothesis. Latent motion has been imagined to be no notion at all. But while on the one hand there is the strongest reason to doubt whether caloric ever exists in more than one mode when it is contained in matter; on the other hand, the motion of latent heat itself is not perhaps altogether incompatible with the theory in dispute. At least some of its advocates are of such an opinion.

There is still another theory of the nature of caloric not less powerfully supported by authority and facts,

than any of the preceding. This hypothesis assumes the identity of light and heat. It supposes that light becoming united with matter exhibits the phenomena of heat. It is well known that all solid bodies shine at a certain temperature. They cannot by any means be heated above this point without shining. But what is the source of this light? It does not arise from the fire employed. For hot air, which itself is wholly invisible, is yet able to communicate to a solid body, as iron, the property of emanating light. Nor is air at all necessary for this effect, since iron is similarly affected in melted lead. Neither can the light arise from the shining body itself. For if this were true, by continuing the process sufficiently long, the solid substance would at last lose its property of shining, having given out all its stock of condensed light. But, on the contrary, however long a body is made to shine, it appears still as bright as at first, even after an interval of months. If then the light emanated from a hot body was not contained in it when cold, and does not proceed from the fire, at least in the form of visible light, what other source is left for it? Plainly none, unless the caloric of the air, or other substance communicating heat to the shining body. Whence the inference should be, that caloric of a sufficient intensity is radiated from hot bodies in the form of light: in other words, that light is caloric. Heat in almost all cases produces light, and light, in return, very frequently produces caloric. If instead of saying fre-

quently, we were borne out by facts to say always, there would remain very little doubt of the identity of light and heat. But this theory does not want, more than those already stated, its own share of difficulties. It has not been found possible, hitherto, to heat gases to the shining point. But these bodies ought to shine as well as others, if heated to the proper degree. Whether they require a very high temperature to enable them to shine, and are merely to be considered as bad radiators of heat or light, future experiments must determine. It is clear, however, from the ingenious experiments of Mr. Leslie, that bodies do vary in that quality very much from each other. On the other hand, though the solar beam, and the rays of terrestrial fires, excite heat in the bodies on which they fall, the rays of the moon can by no means be shewn to produce a similar effect. This has been attributed to the extreme tenuity of the lunar rays, which are supposed to be many thousand times more rare than those of the sun. Whereas, the highest powers of convex lenses have not been able to concentrate these rays so much, as to render it likely in theory that heat should be so produced.

If these two objections could be obviated, the hypothesis of the identity of light and caloric would be considered as the most probable yet proposed, concerning the nature of caloric.

These are the principal, or most probable theories, which profess to explain the phenomena of heat.



From the number of arguments which can be adduced for the support of each, one inference may be not unfairly drawn: That philosophers are yet very far from being completely acquainted with the intimate nature of the subject of the present inquiries, and that none of the hypotheses is free from solid objections. In reality, the subject is by no means completely investigated. But though the cause thus eludes our efforts, considerable progress has been made in arranging and explaining the facts; many curious and important phenomena have been discovered and investigated, and chemistry, with its dependant arts, has received solid benefits from the experiments and researches which have been undertaken for the purpose of establishing or confuting one or other of these theories.

The production or collection of caloric by friction is an extremely curious phenomenon. In those cases in which the body remains unaltered, as to what is termed its capacity for heat, and in those where it does not affect the state of the atmospheric air, there are great difficulties in every way of accounting for the rise of temperature. Count Rumford has shewn, that iron, exposed to violent friction, will become exceedingly hot, even under water, though there appears no cooling of the surrounding bodies. Caloric indeed may be here said to have flowed in from the surrounding matter, the place of each portion being immediately supplied by the caloric behind, in a similar manner to that by which water

rushes to replace any portion removed, each particle occupying successively the situation of that before it. In this case caloric may be supposed to be collected by friction, as electricity is on the surface of glass and other electrics by similar means. It might be curious and would not probably be very difficult to try whether the conducting powers of bodies for caloric have any relation to the increase of their temperature by friction. Perhaps if a body could be as completely insulated from caloric, as it can be from electricity, friction would not heat it.

Although it may be difficult to point out with due precision the exact nature of the supposed motion amongst the particles of bodies which produces the phenomena of caloric, yet we are still too little acquainted with the properties of matter to be able to deny the possibility of such a motion. It is generally supposed, that the ultimate particles of bodies do not touch each other, for proof of which it is assumed that almost all substances contract their bulk when their temperature is diminished. But if the particles of matter were already in actual contact, they could not approach any nearer to each other. And as the definition of an ultimate particle supposes a portion of matter which no longer admits of division or penetration, the particles themselves cannot be supposed to dilate or contract their dimensions. Hence arises the inference that the particles of bodies do not touch each other. In this argument, however, there are some objectionable

points. The definition of a particle which denies the possibility of its contraction or dilation is itself without proof, and is moreover not very susceptible of any. It cannot be a very easy task to shew that there are ultimate particles which are not themselves distensible by caloric, but amongst which this substance or quality penetrates, and, either by its chemical or mechanical power, separates them. But even if this were granted to be true, it does not seem a necessary consequence that the particles do not touch. It certainly must follow that they do not touch at every point. It is probable, however, that a partial contact of particles might be made to agree tolerably well at once with the expansion of bodies by heat, and with the impenetrability of the particles themselves.

The connexion between caloric and light, if indeed these are different bodies, is very obscure. But it is well known that most bodies shine, that is, emit rays of light, very copiously at an elevated temperature. It has been an object of curiosity to ascertain the point of the thermometer at which this takes place. But in temperatures so high the mercurial thermometer cannot be applied for obvious reasons. To supply this deficiency a rule was proposed by Sir Isaac Newton for calculating the progress by which hot bodies cool, and thence deducing the temperature at any period in the process. That illustrious philosopher imagined that the heat lost by any body at a very high temperature in equal small times are as the heat existing in it, rec-



making the heat in the body equal to its excess above that of the atmospheric air in which it is placed. That is to say, taking the times in arithmetical progression, the portions of heat lost in these times would be in a geometrical one. Thus if the body were  $64^{\circ}$  hotter than the air, and in the space of five minutes lost  $32$  degrees; in the next five minutes it would lose  $16^{\circ}$ ; for  $64 : 32 :: 32 : 16$ . And in the five minutes before the application of the thermometer it would lose  $64^{\circ}$ ; for  $16 : 32 :: 32 : 64$ . In the same manner if a body  $180^{\circ}$  above the heat of the air were observed in five minutes to lose  $60^{\circ}$ , or was only  $120^{\circ}$  above the temperature of the air, in the next five minutes it would lose less in the proportion of  $180 : 120$ : that is  $40^{\circ}$ , and the whole heat now remaining will be  $80^{\circ}$ . In the next five minutes it would lose less in the proportion of  $120$  to  $80$ , or  $26\frac{2}{3}$ . And in the five minutes prior to the first observation it would lose a quantity greater than  $60^{\circ}$  in the proportion of  $120 : 180$  or  $90$ , and in the five minutes preceding it would lose  $180 : 270$ , or  $135$ , and so on. Now  $26\frac{2}{3}$ ,  $40$ ,  $60$ ,  $90$ ,  $135$ , are terms in geometrical progression. This is a most ingenious method of determining those very high temperatures to which no thermometer can reach, and it is found by actual experiment to agree very nearly with the phenomena, the differences not being greater than must arise from unavoidable inaccuracies. The principle however is assumed. This method is of con-

siderable practical utility, and was first applied\* by Dr. Irvine to calculate the heat lost during the first minute in experiments for determining the absolute heat of bodies.

It is not, however, free from objections ; for if the law of the decrease of heat was such that the degrees of heat lost were as the heat subsisting in it, a body, though constantly cooling, would never arrive at the temperature of the surrounding medium. For if the degrees of heat lost in equal times be 8, 4, 2, 1, then according to the hypothesis the order of cooling after this would be  $\frac{1}{2}$ ,  $\frac{1}{4}$ ,  $\frac{1}{8}$ ,  $\frac{1}{16}$ ,  $\frac{1}{32}$  in infinitum. But as we find that a body cooling in a very little time arrives at the temperature of the air, this cannot be the case.

Dr. Martin, from a number of experiments, is of opinion that the law of decrease of heat is compounded of two series : in the one, and that in general of much the greatest consequence, they are always in proportion to the heats themselves, or in a geometrical progression as in Sir Isaac's theory. While in the other series, indeed the least material, the decrements are as the times, or always uniform, that is, equal quantities of heat are lost in equal times. This law of the decrease of heat is the same with that of the retardation of a heavy body ascending perpendicularly in a medium which resists it in proportion to its velocity.

\* Crawford on Animal Heat.

But though the Newtonian theory be liable to these objections, of which its illustrious author could not be sensible, yet he never intended that it should be applied to determine the law of the cooling of bodies near the temperature of the air. He proposed it only for ascertaining great heats out of the reach of the thermometer, and for this purpose it answers perfectly well. The improvement of Martin does not affect this computation. In high temperatures a few degrees of error are of small import in the calculation, since the mistake of a second or half a second in the times would affect it much more, and this method has the further disadvantage of requiring a troublesome computation.

Sir Isaac Newton calculated according to his own hypothesis various temperatures above the reach of the mercurial thermometer. He found that iron is just visible in the dark at  $635^{\circ}$  of Fahrenheit; that it shines strongly at  $752^{\circ}$ ; that it is luminous in the twilight at  $884^{\circ}$ ; and that when it shines in day light its temperature is above  $1000^{\circ}$ . Dr. Irvine turned some attention to the investigation of this subject, and obtained results differing a little from those of his illustrious predecessor, employing another method of arriving at the same point, founded on the doctrines of heat proposed by Dr. Black. He observed that the point  $635^{\circ}$ , at which Sir Isaac imagined bodies to shine in the dark, seemed not to be far from the truth. For mercury, which boils

nearly at this temperature requires, about a red heat to make it distil. But according to some experiments which he made, the degree of heat at which bodies appear luminous falls somewhat higher. He found that with a mercurial thermometer most accurately graduated, mercury boils at  $672^{\circ}$ . He next proceeded to try whether boiling mercury shone in the dark; every precaution was taken to ensure the accuracy of the experiment. In order to make the determination accurate, proper assistants were procured, some of whom were placed in a room perfectly dark, in which they remained for some time that the eye might become as sensible as possible. The room communicated with another by a hole in the wall with several turnings, so that no ray of light could possibly pass from the one apartment to the other. Through this hole the mercury which was boiling in the light room, was handed into the dark one, for examination of its power of emitting rays of light, but it could not be at all perceived. From this it was inferred that the shining point was above  $672^{\circ}$ . The means, however, of determining whether a body shines or not at a certain temperature are not perfect. The only test of accuracy is the human eye, and it is well known that different eyes vary very much from each other in their power of vision, and even the same eye at different times and in various circumstances. When the mercury at  $672^{\circ}$  was introduced into the dark room, although the

persons present did not see it, a cat, which was there, seemed to have some perception of it, walking round the vessel in which it was contained. This however might have been a feeling of heat as much as a perception of light. The mercury in the thermometer may be supposed not to be a perfectly accurate measure of the degree of heat at which mercury boils, on account of the irregular expansions of most bodies at high temperatures. But it appears a perfectly fair and reasonable induction that at whatever degree of the thermometer mercury boils, the luminous point must be somewhat higher, at least of that body, and probably also of all others, since there seems reason to believe that this effect of heat is produced in all bodies that are capable of it nearly at the same degree.

Since then it appears, that  $635^{\circ}$  proposed by Sir Isaac Newton is too low for the point of visibility in the dark, it became an object of some curiosity and importance to arrive at a more accurate determination. Dr. Irvine found by numerous experiments, that equal bulks of iron and water of different temperatures being taken, and the iron immersed in the water, the new temperature is *nearly* the mean between the former ones. He heated iron of a known bulk to a proper degree, it was then introduced into the dark apartment through the orifice already described, and it was allowed to cool till it just disappeared. At that moment it was dropped into water of a given bulk and temperature, and

the heat was observed which it communicated to the water. If the quantity, by bulk, of water was ten times greater than that of the iron, he computed that the iron had been ten times hotter than the heat which it imparted to the water. That is, if the water had gained one degree, that the iron must have lost ten degrees; or in other words let the water be taken as ten and the iron as one, and for every degree given to the water allow ten degrees to have been in the iron. Proceeding in this manner, Dr. Irvine made many experiments to ascertain the luminous point, but I have not been able to learn accurately the numerical result.

In a similar way he made many trials of the heat of a common coal fire, by examining the temperature of a piece of iron which had been allowed to remain in it for a considerable time. In no less than twenty successive experiments, instituted to ascertain this point, Dr. Irvine never found the result higher than  $796^{\circ}$ , or lower than  $790^{\circ}$ . This coincidence, which in experiments of this kind is truly remarkable, may be regarded as nearly exact, and adds much to the probability of the justness of the method employed. The sources of inaccuracy also seem chiefly to have been such as rather lower the result. Such for example as the steam, which might be formed, the heat necessary for which ought to have been added to raise higher the shining point. Such errors, however, in carefully conducted experiments, are not so great as might be expected, as will in



some measure appear from some calculations in the third part of these Essays, concerning similar losses by steam. Dr. Irvine, by varying his experiments; illustrated the degree of accuracy to be attributed to this method. He determined the melting point of lead by the mercurial thermometer to be  $594^{\circ}$ . He allowed a piece of red hot iron to cool till it was just hot enough to melt a piece of lead put upon it, and then trying its temperature by immersion in water, he found it not above a degree different from the thermometrical determination. In opposition to these concurring circumstances, there is only one reason for suspecting the point to be too high; that is, because when any substance acquires softness, it tends to fluidity, and absorbs latent heat. But after all allowances for this softness the point of ignition cannot be reckoned lower than  $780^{\circ}$ .

Dr. Irvine employed this same process to ascertain many lower degrees of temperature, such as the melting points of tin, zinc, &c. And he continued to remark a surprising and gratifying coincidence between the results thus obtained and the same points when he examined them by the test of the mercurial thermometer. There seldom appeared more than three or four degrees of difference between the two modes of observation and computation.

Caloric is well known to have a strong tendency to diffuse itself in all directions, nor can it be retained in any body otherwise than by the juxtaposition of other substances still more loaded with heat.

Although caloric has been clearly proved not to enter into all bodies with equal facility, or in the same quantity, yet a hot and a cold substance in the immediate vicinity of each other very speedily acquire the same temperatures. This property has been distinguished by the name of the equilibrium of heat, and various attempts have been made in this island and by our continental neighbours to invent some hypothesis which might afford an explanation of it. Caloric has been supposed to be a fluid of a very subtle nature pervading all matter, as water penetrates the vacuities of a sponge; and hence, when accumulated in larger quantities in any particular body than in those surrounding it, that there was a tendency in the caloric to flow out of the hot body into the colder ones. This tendency, however, certainly cannot be supposed to differ from an acting force; for if heat be accumulated in any body it ought to remain there for ever, unless either attracted or repelled from its situation. The case of a sponge floating in water, used for the illustration of this opinion, undoubtedly supposes a positive power. For if water could be accumulated in the sponge, it would only leave that body, being attracted by other bodies, or by the gravity of the earth, or by some supposed elasticity of its own substance. This theory was altered and explained by Piéctet and others, who attributed to the particles of caloric the power of repelling each other, which repulsion increases directly as the distance diminishes between the particles. From this it follows



that caloric is retained by one force, that of the attraction of the body in which it is contained, and that it is discharged by another, that, to wit, of the mutual repulsion of the particles of caloric. Consequently when two equal bodies are placed in the vicinity of each other, and the force of the attraction of one for heat plus the repulsion of the particles of caloric in the second, equal the force of the attraction of the second for heat, plus the repulsion of the first; these two bodies will not communicate heat to each other, that is to say, they are of the same temperature. Another opinion is that of Prevost, who dropping the consideration of the attraction of bodies for caloric altogether, attributes the equilibrium of heat solely to the repulsion between its particles, or, as it is now usual to style it, to the radiation of caloric. This radiation is imagined to be greater or less exactly as the temperature is higher or lower. Bodies therefore mutually heat each other, and temperatures are determined by the difference between the rays given out and those received. Hence clearly an equilibrium of caloric must arise. But this theory seems to me wholly inconsistent with the well ascertained fact that hot bodies cool slower in a vacuum than in the open air. Caloric therefore must be conducted from one body to another, not by radiation only, and bodies must still be allowed to conduct caloric from each other without the intervention of radiation.

This theory of Prevost has much analogy with the

ingenious Hutton's opinions on the same subject. Dr. Cleghorn also appears to have viewed the phenomena of caloric in a manner extremely similar to the hypothesis of Pictet. Upon the whole, it may be remarked, that none of these theories are of extremely recent invention, unless in some few points, where additions or corrections have been made. The last hypothesis, which would attribute the whole motions of caloric to radiation, has been lately opposed in a series of new experiments by Mr. Leslie. In particular, he has observed that the agitation of the air through which the caloric is radiated very much impedes its progress, a fact corresponding with his own opinions and illustrative of the carrying power of air, but not easily reconcileable with the previously entertained ideas of radiation. On this subject however it is perhaps not yet possible to decide with propriety till other philosophers have repeated these *experimenta crucis*, or till the adherents of the doctrine of radiation have had time to institute other trials to determine the truth of these new opinions.

If it should be allowed that every additional dose of caloric with which any body combines is held by a weaker affinity than that before admitted, which must be true if the chemical union of heat with matter be granted at all, and if the particles of caloric repel mutually the more strongly as they approach nearer to each other, a question arises, can caloric be added to any body *ad infinitum*? Are there any bounds to the possible augmentations of temperature?

It appears reasonable to say that there must be a limit. For the attraction of the body for caloric is constantly diminishing, and the repulsion between the particles of caloric as constantly increasing. That is to say the means which enable the body to take in caloric, are taken from it gradually, and the power of the caloric to escape from confinement are progressively enlarged. There must therefore be some point at which no more heat can enter, some point at which an equation shall be established, the two opposite forces mutually balancing each other. A body at this temperature can no longer admit heat, and cannot even be retained at the same point, unless all other bodies were equally warm. For though surrounded by any number of equally hot substances, if there existed one colder body without the circle, the caloric, however gradually, would infallibly be dispersed in that direction. It does not seem likely that any practical use could be made of this inference, admitting it to be just. If, however, any experiments could be devised, by which there could be determined the ratio in which the attraction of any body for successive doses of caloric diminishes, and on the other hand the ratio in which the mutual repulsion of the particles of caloric increases, perhaps we might be enabled to calculate the highest degree of heat to which that body would be raised under the influence of these two powers. A third power, no doubt, might be conceived to possess some share in the arrangement. I mean the force by which caloric might be repelled from a very hot

substance into one at a lower temperature, the strong repulsion of the caloric in the hot body overcoming part of the weaker repulsion of the cooler. This, however, would have its limits, which might be determined on the same principles as before mentioned. If we could conceive caloric to be so condensed that its repulsive powers would exceed those of the caloric in a body at the highest degree of heat, computed as has been proposed, a body surrounded by such caloric would be forced to admit it in greater quantity, and by thus supposing *à priori* that caloric is so condensed, there would be no limit of the entrance of heat into a body. If, however, the repulsion of the particles of caloric increase inversely as the squares of the distances, or in any other ratio, the condensation of caloric must have a limit, and nothing would account for a higher temperature but the gratuitous supposition of a body already so heated by unknown means.

If there were any certain foundation for such a calculation as has been now imagined, which I am far from supposing to be the case, it would be possible, by inverting the process, to determine also the lowest degree of heat to which any body could be cooled. But even if all this were just in theory, there would be little reason to hope that we should ever be enabled to procure practical results, or to compute with due accuracy either the ratio according to which bodies have their attraction for caloric diminished, or that by which the particles of caloric proceed to increase their repulsion for each other,



## ESSAY II.

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### *ON SOME OF THE PRINCIPAL DISCOVERIES MADE BY HELP OF THE THERMOMETER.*

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No branch of human knowledge has received more extensive additions from the invention and use of an instrument calculated to facilitate and improve the methods of observation than the science of heat has derived from the employment of the thermometer, under the influence of which the progress of this department of philosophy has been wonderfully accelerated, and the theories of the nature of caloric subjected to the ordeal of a severe and accurate reference to experiment. The properties of caloric indeed were little attended to or understood before the invention of thermometers. All that was known concerning it was derived from the observation of its effects upon the human body ;

a source of knowledge liable to great inaccuracies from various causes. The same substance, by a very simple experiment, may be made to appear hot to one hand, and cold to the other. And in many diseases, where the patient has a strong sensation of cold, it frequently happens, that the body is then warmer than it ever is in a healthy state. The very sense of feeling too is liable to be destroyed, at a temperature a little above or a few degrees below that of the human body. Hence all such observations are necessarily confined within these limits. This measure of caloric, therefore, is merely relative; whatever is warmer than the body at the time appearing hot to it, and whatever is colder appearing cold. And as the heat of the hand, the principal organ by which determinations of this kind are made, is perpetually varying, the substance examined is pronounced hot at one time and cold at another, though its temperature be really the same in both cases.

The invention of the thermometer superseded all the former vague and uncertain methods of estimating the degrees or qualities of caloric. It shewed that all kinds of heat were the same, and differed from each other only in intensity, an opinion very dissimilar to that entertained formerly, when the grandeur or baseness of the source was supposed to influence and alter the nature of caloric itself. Experiments also made in one country could be repeated and extended in another, when the thermometer was employed; and a degree of accuracy was introduced



into the philosophy of heat, of which the subject had not been imagined to be susceptible. For the construction of these instruments, after various trials, the preference is now given to mercury; a substance which does not freeze but with extreme cold, nor boil till raised to a very high temperature, affording, thereby, an extended interval, for the mensuration of the degrees of caloric. Its cohesive attraction moreover being very great, it is free from the inconvenience of adhering to the sides of the glass tube. Besides which, its expansions have been proved, by the experiments of Black, De Luc, and Crawford, to be, within certain limits, the best measure of heat that we are possessed of; that is to say, it expands equally with equal increments of temperature. Not that it is entirely or perfectly accurate, but that it approaches accuracy nearer than any other known substance, and that within the limits of its intermediate expansions, and avoiding the temperatures which border on its freezing and boiling points, its errors are probably not of great practical consequence. †

The computation of degrees ought to begin from the natural zero or point of privation of all heat. But this point not being sufficiently known, the zero has been placed at the freezing point of water, by almost all the philosophers of the continent, and by several of our own country. Notwithstanding objections urged for some time, against the permanency of the freezing point of water, it seems upon the whole to be by far the most unexceptionable point for

the commencement of the thermometric scale. And it accordingly makes the zero in the thermometers of Reaumur and Celsius chiefly employed in Europe. Fashion, more than any circumstance of superiority, has induced in this island the preference of a thermometer on the principles of Fahrenheit, who placed his zero at the greatest degree of cold which he had been able to produce by a mixture of snow and common salt, and which was once supposed to be the greatest possible cold. These thermometers, however, are now universally constructed and graduated by the help of melting ice or snow, which is found a far easier and more certain method, marking 32 at that point. By the experiments of Mr. Walker, of Oxford, however, it appears, that the thermometer has been sunk several degrees below Fahrenheit's zero by the mixture of snow with sea salt;\* and long before Mr. Walker's experiments, Dr. Irvine had produced in this way, a cold equal to  $-9$  of Fahr.

About the middle of last century, by help of the instrument, the merits of which we have been discussing, some observations were made and some consequences deduced, which have produced unforeseen

\* This experiment was performed in the following manner:  
" the temperature of the air being  $34^{\circ}$ , an ounce of sea salt at  $34^{\circ}$   
" was mixed with fourteen drachms of snow at  $31^{\circ}$ , the temperature  
" produced was  $-7$ ; much of the salt was still solid. I poured off  
" the clear fluid, and mixed more snow at  $28^{\circ}$  with the moist salt;  
" the temperature produced was  $-7$ . Repeating this experiment  
" with a greater quantity of snow and salt, the cold was  $-9$ ."

and immense changes in the whole science of chemistry, and conferred immortal honour upon the illustrious discoverer. Dr. Black, to whom this merit belongs, made these experiments and deductions about the year 1755. M. De Luc has claimed an equal share of the honour of these discoveries, to which, however, if we believe the ingenious Dr. Robinson, in his notes upon Dr. Black's Lectures, he has very little title. It has been long ago observed, that ice, when it is heated to 32 Fahrenheit, suddenly stops in its rise of temperature, though the temperature of the surrounding bodies be higher than 32, and pertinaciously continues at the same point till the whole is melted. No intelligible explanation, however, had been offered of this fact, which indeed, in a great measure appears to have escaped the notice or to have attracted very little of the attention of philosophers. It might be demanded, did the melting ice admit heat or not? If it did not, what strange cause prevented the caloric from entering into the ice at that point alone? If, however, it did, how are we to explain the admission of caloric during so long an interval without any corresponding rise of temperature. It would appear, either that nobody thought of answering these questions at all, or at least that nobody was able to propose a just and satisfactory account of the matter. Such was the state of philosophical opinion concerning this subject at the time of Dr. Black's first attention to it. That gentleman, having considered the state of the theories and knowledge

of caloric at that period, resolved to submit to the test of experiment, whether heat did or did not enter into melting ice. And he clearly shewed that the ice did not refuse admission to the caloric as some imagined, but on the contrary, received an unusally large quantity. He observed that a vessel with ice, though it did not acquire a higher temperature, while any part of the ice was unmelted, certainly admitted heat, since a stream of cold air descended from the vessel during the whole process of fusion. He observed further, that a pound of ice and a pound of water each at  $32^{\circ}$ , being each separately mixed with a pound of water at a higher temperature, that the mixture of the two pounds of water had a temperature nearly the mean of their former temperatures; but, that when ice was used as one of the ingredients, the temperature of the mixture was lower than the mean, and that  $140^{\circ}$  seemed to have disappeared, or to have entered into the ice during its conversion into water. These 140 degrees were styled by Dr. Black latent heat, because, according to his view of the subject, the thermometer gave no indication of their presence, which was only to be discovered by the change of form from the solid to the fluid state. This great discovery opened a new and prodigious field for the labour of philosophers. Dr. Black soon saw reason to extend his doctrines of latent heat to all cases of fusion; and Dr. Irvine, then his pupil, experimentally determined its truth in several instances. He found the latent heat of spermaceti to be about  $145^{\circ}$ , as

measured by the capacity of melted spermaceti; that of bees' wax about  $175^{\circ}$ , and that of tin  $500^{\circ}$ , as measured by the capacity of solid tin. Dr. Black applied the same method of explanation to vaporizations of every kind, and shewed the necessity of large supplies of heat to the existence of steam, and other bodies in a state of vapour, by experiments convincing and unexceptionable, but which it is now needless to detail, when the fact is universally admitted. Dr. Irvine by experiment, made the latent heat of steam amount to about  $800^{\circ}$ , by a method such as is detailed in Dr. Black's Lectures, though some particulars do not altogether coincide with the manuscript account of the experiment in my possession; Mr. Watt afterwards computed the latent heat of steam to be from  $900^{\circ}$  to  $950$  degrees; M. Lavoisier supposes it to be as high as  $1000$  degrees.

Dr. Black thus established the truth of a general and most important law of nature, that all bodies require for their fusion and evaporation an immense, and formerly unsuspected quantity of caloric. To the knowledge of these facts, it cannot be doubted, that we owe much of the modern advancement in chemical theory.

It has been already mentioned, that all known bodies contain, and more or less freely transmit heat. This fact is indisputably true. But when it is next inquired, if they contain equal quantities in equal bulks, or in equal weights, or in any other way, the answer is not so clear. It was supposed by Dr.

Boerhaave, that bodies contain heat in proportion to their specific gravities. But Dr. Black shewed this opinion to be unfounded, and that bodies had powers of containing heat peculiar to each peculiar substance, and that not connected either with the comparative weights or bulks of the substances examined. This power of being unequally heated by equal quantities of heat, has been the object of considerable inquiry, and was distinguished by Dr. Black and Dr. Irvine nearly about the same time, by the term capacity. The same meaning has been expressed by various other terms, as comparative and relative heat, specific heat, equilibrium, &c. If any body be heated a certain number of degrees by a given quantity of caloric, and its capacity be called 1.000, and another body be heated twice that number of degrees by the same quantity of caloric, then its capacity must be expressed by .500, or be only one half of the former, because each degree of its temperature is raised by a portion of caloric only half of that which is necessary to raise each degree of the others. The capacities of bodies may thus be numerically compared, and this is generally done by calling the capacity of water 1.000, and comparing that of all other bodies to it. It is not possible to apply directly to any body a certain quantity of caloric to make this trial, for caloric is inseparable from matter, unless the case of radiant caloric be considered as an exception to this, and even then, it is impossible to apply any measured quantity of caloric to a body. But it is



easy to observe, how much heat, equal quantities of different substances subtract from a given quantity of any matter, water for example, to have their temperature raised to the same degree. And this is in reality, to do the same thing, as if we could separate the caloric from all other matter, and apply it in measures or weights to the substances under examination.

The discovery of these properties of caloric, has entirely altered men's opinions on that subject. New roads have been opened for the progress of philosophical inquiry, and a degree of precision, and of truly useful and elegant knowledge, has been obtained, which there was formerly no reason to have expected.

All who have followed the footsteps of Dr. Black, in adding further to the facts or the theories concerning caloric, have begun by acknowledging the truth and the importance of his discoveries. As far as I know, no person has attempted to invalidate his observations, which are indeed so clear, and supported by experiments so simple and decisive, that they enforce belief. Whatever additional facts may have been discovered, or whatever variation of theory may have been advanced, his merits cannot be thereby affected.

From the facts already stated, it appears that the fusion, or melting of bodies, is not performed altogether in the manner that was at one time imagined. It is not enough, merely to add such a quantity of heat as would have been sufficient to raise the tem-

perature of the solid above the point at which fusion takes place, but a much larger supply is required than is necessary for a similar elevation of temperature either before or even after the liquefaction. In other words, a portion of caloric enters into the body that is fused, without raising its temperature. This quantity of heat, which in the case of water, amounts to about 140 degrees, for there is some difference of result in the experiments for ascertaining this point, was called by Dr. Black, latent heat, and has received from some other philosophers the name of caloric of fluidity. It was asserted by Dr. Black, to be the cause of fusion, and to combine chemically with the solid, and form a substance differing both from the solid and the caloric, to wit, the fluid.

This opinion has been the cause of very great discussion, and was in particular by Dr. Irvine supposed to be, if a just, at least, not a sufficiently comprehensive view of the subject. He was not disposed to consider the entrance of what is called latent heat into bodies as happening upon different principles from those which always direct the operations of caloric upon matter. Though ready to admit the discoveries of Dr. Black, in all their extent and importance, and no man thought more highly of them, as none more fully appreciated their value, he imagined that latent heat was only a case of what occurred in every affection of bodies by heat, and that the caloric existed there precisely in the same way as at other times, and could be discovered by the same

tests which at any time give notice of its presence. Not willing to descend to a dispute concerning a term, he was ready to admit the phrase, latent heat, as the expression of a new and curious fact, though not without some modification in the exact sense to be attached to it. He was accustomed to observe, that the latent heat followed as a mere consequence of his peculiar view of the operations of caloric; by this assertion, not claiming any share in the honour of discovering the existence of latent heat, but expressing that his theory being granted, the previous discovery of Dr. Black fell into it as a part of a whole.

Dr. Irvine considering the phenomena of latent heat in comparison and in combination with the discovery of the different capacities of bodies, was led to suppose that they might mutually illustrate each other, and that something more might happen during fusion than Dr. Black had imagined. All bodies, it appears, are differently affected by the same quantity of caloric. No two substances shew the same augmentation of temperature by equal increments of absolute caloric. To raise any body from one given thermometrical point to another, requires an addition of heat, which must be greater if the capacity of the body be greater, and less if it be less: this quantity then is proportional to the capacity, as all experiments have shewn, as well in the lowest as in the highest temperatures in which it has been possible to make the trial. If then we suppose that any substance were totally deprived of caloric, yet retained its power of receiving or com-

bining with it; it appears reasonable to say, that a certain quantity of caloric applied to this body, would raise its temperature by a number of degrees proportional to its capacity. And, if this capacity were then increased, that a greater quantity of caloric would be required for producing the same effect or elevation of temperature.

To the best of my information, it was by a process of reasoning somewhat similar to this, that Dr. Irvine was induced to conceive that it was possible that the capacity of water might be found to exceed that of ice; upon experiment being made, that if this really turned out to be the case, a new reason might be assigned why ice during its melting refused to admit of an augmentation of temperature. He imagined therefore, that a larger quantity of caloric might be necessary to raise the temperature of water to 32° than that of ice to the same degree, supposing the heating of each to begin at the point of total privation of all heat. In this case, the heat could not be called latent exactly in Dr. Black's sense, since the thermometer shewed, that though the number of degrees was the same in both instances, the value of each degree was different. All bodies therefore should contain heat in a similar way, and by a like mode of union, whatever that might be.

It is not often that I am able to express in Dr. Irvine's own language, the circumstances of his theory. But I now subjoin a passage from his manuscripts explanatory of this subject.—  
“ The reason why a body cannot be heated while

it is melting is, that it is then changing its capacity. From a substance that was easily heated, it changes to one that is heated with difficulty. All bodies in a fluid state seem to be heated with more difficulty than when in a solid state. Not that a body in a fluid state transmits heat more slowly than when the same body is in a solid state. On the contrary, it transmits heat better in a fluid than in a solid state. But the same quantity of heat applied to the same body in a solid form, and in a fluid state, will heat the solid a greater number of degrees than it will the fluid. *Exempli gratia* : if a quantity of absolute heat 10 or X be applied to a solid body, and is capable of heating it  $20^{\circ}$ , reduce the solid to a fluid state, and this quantity of heat will only be capable of heating it  $10^{\circ}$ . Therefore, if all the heat in the body in a solid state was  $20^{\circ}$ , and if it became all instantly fluid, the whole sensible heat in it, in this case, would only be  $10^{\circ}$ ; or the body would have become instantly ten degrees colder without any heat being taken out of it. And if the same body, in a fluid state, became instantly solid, it would immediately become  $10^{\circ}$  warmer without any heat being thrown into it. In these cases, the quantity of the heat in the body does not alter by the change of form. All that happens is, that the same quantity of heat produces a greater effect in heating in the one state than it does in the other."

The first and indispensable condition for the truth of this hypothesis is to sh w, that the capacities of water and ice, or of fluids, and of their relative solids



in general, really do differ in the manner here supposed. With this view, Dr. Irvine was at great pains, and made very many experiments to determine the capacities of ice and water. For this purpose, he used a third body to measure the heating or cooling powers of these two substances, sometimes employing mercury, sometimes iron filings, and lastly, preferring fine river sand washed, or pounded glass for the medium of comparison: these experiments were conducted in the usual manner, and with the usual precautions, though much more than usually embarrassed by the different temperatures of the air, and the substances used. For it is impossible to perform experiments to determine the capacity of ice or snow without reducing that substance to a very low temperature, near the zero of Fahr. or a few degrees only above it. While the air, if many degrees higher in temperature, inevitably communicates during the process a quantity of caloric, which tends greatly to the uncertainty of the results. But temperatures of the air near zero are very rare, and of very uncertain duration in our variable climate. More accurate results, therefore, might be reasonably expected, if such experiments were repeated in countries where extreme colds prevail, and where of consequence, both longer time and better opportunity would be given for determining this point. From the means of many trials made at various favourable times by Dr. Irvine, he concluded the capacity of water to be to that of ice in a ratio not greater than 10 to 8.



Dr. Crawford found the proportion to be as 10 to 9 nearly, and Mr. Kirwan, if I do not mistake, stated the numbers to be 10 and 9.

Considerable anxiety has been expressed to learn the exact manner in which Dr. Irvine conducted these fundamental experiments, upon the truth of which, so much of his theory depends. Many philosophers appear to have imagined them to have been performed by a mixture of ice and water, which is altogether untrue. Such a thing may, perhaps, be considered as theoretically possible, but practically of extreme difficulty. Capacities are solely the expression of the rise or fall of temperature, produced by equal quantities of caloric upon different bodies. If, then, we could mix ice of a very low temperature with water, so that no water should be frozen or ice melted, we should have ground for determining their capacities for heat. The difficulty here would be to determine whether any ice was melted or not: other methods might be proposed upon similar principles, but equally difficult or impossible in execution.

Dr. Irvine had not recourse to this process. I have already given the outlines of his plan. He found the capacities of some suitable bodies, as river sand, or iron filings, and compared them with that of water in the usual manner. This being done, he used the same body to examine the capacity of pounded ice formed from distilled water, or of snow. The temperature of the room and vessel was, in his experiments, always either  $32^{\circ}$  or below it; most commonly

considerably under  $32^{\circ}$ . He then took a known weight of snow or ice of a known temperature, in a vessel of which the capacity was determined by experiment. Upon this he poured a certain quantity of river sand washed, or iron filings of a certain temperature, with as much rapidity as possible: the new temperature was observed after stirring, and allowance was made for the heat gained or lost: the temperature of the mixture was frequently  $19^{\circ}$ ,  $20^{\circ}$ ,  $25^{\circ}$ ,  $16^{\circ}$ , &c. So that in a room where the air was below the freezing point, the accuracy of the result could not be affected by the formation of any water; still there are many sources of inaccuracy remaining. But in Dr. Irvine's hands, the capacity of ice always turned out to be less than that of water. In all his experiments, which were very numerous, and repeated with care for many succeeding years, he arrived at results approximating to each other, and concluded, to use his own words, that from the mean of all his trials, the capacity or relative heat of water to that of ice is not in a ratio greater than 5 to 4 or 10 to 8. To this I may only further add, that the near coincidence of the experiments of different philosophers at different times and places, affords fair grounds for supposing that all of their determinations approach to accuracy. A praise the highest that any reasonable man can ever expect to obtain for experiments of so delicate a nature.

In like manner, Dr. Irvine extended his theory to all other bodies whatever, and in some cases deter-

mined and in all inferred, that it is a general law of nature that the capacity of all solids, for heat is increased by fusion, and that of all fluids by vaporisation. This law, the existence of which was not before even suspected, must be considered as an highly important observation, as all generalisations of facts are to be regarded, and that whether the theory which is attempted to be deduced from it be ultimately established or overthrown.

It appears then, that besides Dr. Black's discovery of the great quantity of caloric necessary for the conversion of a solid substance into the fluid form, another remarkable alteration is produced in the habits of the body with regard to heat. It is changed from being a body, which is easily heated, or which requires only a small quantity of caloric to raise its temperature by a certain number of degrees, to be a body difficultly heated or requiring a greater quantity of caloric for producing that effect. But if this change in the body could by any means be suddenly induced, it appears highly probable, if not absolutely certain, that the temperature of the body must either immediately sink considerably, or that a large portion of caloric must enter it to prevent that consequence. Therefore, in whatever manner fusion may take place, the fluid formed must, from the nature of the change of its capacity, immediately require a supply of caloric to enable it to remain only at the same temperature with the surrounding bodies. It is clear, that if a solid body be converted into a fluid of an

increased capacity, it must absorb heat, which will exhibit the very phenomena that latent heat displays. These arguments apply with equal force to the illustration of the circumstances observable during the conversion of a fluid body into the aëriform state.

If this period were not a little too early for introducing the explanation of the rise or depression of temperature produced in almost every chemical mixture, circumstances illustrative of the present topic might be readily drawn from that source. Of these, however, we shall treat a little farther on. But, though the cause of liquefaction may perhaps bear much analogy to the changes mutually effected by chemical agents, it is not absolutely necessary to refer to these facts for the explanation of this subject.

Various objections have been brought against this mode of explaining fusion and vaporisations. It cannot, however, at least in my opinion, be said to be altogether opposite to Dr. Black's explanation of the same phenomena, but rather is additional to it. Both theories admit alike, as the groundwork of their superstructure, the entrance of a large quantity of caloric during fusion. Dr. Irvine's hypothesis differs only in offering an explanation of the newly discovered fact, the enlargement of the specific heat of the fusing bodies, and in denying any peculiar or unusual combination of caloric in these circumstances.

Dr. Black, however, was not disposed to allow the justice of this modification or alteration of his own

theory. He said, that he supposed the capacities of ice and water really to be to each other as Dr. Irvine represented them, and certainly had no claim either to the suggestion or performance of the experiments made to determine these points. But the inferences did not appear to him to be correct. To quote the Doctor's own words:—"To this statement (of Dr. Irvine's) I answer, that the alleged fact of disparity between the capacity of ice and water, for heat may, indeed, be supposed to account for the thermometrical phenomena just now recited; but the principal fact or phenomenon is not accounted for by it, I mean the change of the solid into the fluid. Solidity, we all know, depends on adhesive attraction; but on what cause does fluidity depend? Will it be said to depend on the absence or cessation of cohesive attraction? I cannot imagine how cohesive attraction can entirely cease, or be suspended; or, if this should happen, how it should easily be again restored: nay, we have evident proof, from the round form of the drops of liquids, that it is not then entirely suspended, but only weakened to such a great degree, that the solid hard body is become a liquid. Now this is such a violent change, that I cannot help thinking it must depend on some powerful cause. It may be said, perhaps, that no other cause is necessary but the increase of sensible heat above a certain degree, together with some singularity in the nature of cohesive attraction, such as that this attraction, though very strong at certain small and

imperceptible distances, becomes suddenly very weak, when we increase beyond a certain limit the distance of the particles of matter from one another, and this increase of distance is produced by sensible heat.

“If this were true, the increase and diminution of distance, by the action of sensible heat, ought always invariably to produce, each its appropriate effect, on the power of the cohesive attraction; as water, when its degree of heat is above  $32^{\circ}$  is always liquid, so when its sensible heat is below  $32^{\circ}$ , it should always be solid. But this we know is not always the fact. Water, in some circumstances, can be cooled to 7 or 8 degrees below  $32^{\circ}$  without being congealed; and many other substances exhibit the same phenomenon, by retaining the fluidity in some circumstances after their sensible heat is diminished, considerably below their ordinary congealing point. And I now ask, what is the cause which hinders the cohesive attraction from producing its effect, and changing the liquid into a solid? When I find by experience, that upon disturbing such over-cooled liquids, a quantity of heat is extricated from them, which did not appear immediately before, and that, while this heat is extricated, a proportionable part of the liquid congeals; I cannot help considering this latent heat which was in it, as having been the cause of its protracted fluidity.”

This kind of reasoning has appeared to Dr. Black, and to many other philosophers, to be conclusive.



Yet it seems to me liable to some very considerable objections, which it shall now fall to my task to state as precisely as I can. And first of all, it must be remarked, that the arguments, now recited, derive a great portion of their force, from the statement given by Dr. Black, and some others, of Dr. Irvine's theory. This statement, however, is by no means such, as an advocate for that hypothesis would chuse to rest his opinions upon. From causes already stated, I have not always been enabled to state my father's doctrines in the precise terms in which he was himself accustomed to announce them. But it appears to me to have been a misconception on the part of those, who have reported his sentiments, to say, that the capacity of the ice is first enlarged, and then a quantity of caloric admitted, distinguished by Dr. Black by the appellation of latent heat. Such a statement at least is by no means necessary for the support of other parts of his theory, and is an awkward inclusion of more points in the explanation, than are by any supposition required. I am at least firmly persuaded, if this were not so, and if Dr. Black's statement were correct, that notwithstanding we are not driven from our ground, but are merely compelled to alter a minute tract of our previous argument. This theory, therefore, may be defended in two ways, either by admitting Dr. Black's as the true statement (and against this almost all objections have been directed), or by taking the account which I have given. That account I briefly recapitulate.

The solid differs from its relative fluid, when both are of the same temperature, in these circumstances, merely, that the capacity of the former is less than that of the latter, and that of consequence, the heating of both beginning at the natural zero, more caloric is necessary for the elevation of the temperature of the water, than for that of the ice. The difference between the whole heat in water at  $32^{\circ}$ , and the whole heat of ice at  $32^{\circ}$  is called the latent heat of that body, and ice being converted into water requires this quantity of caloric to retain its temperature at the same degree as before. But this caloric does not enter the ice before its capacity is changed. Much less is the capacity enlarged before the caloric enters the body. These events are synchronous, and are neither cause nor effect of each other, but are mutually the consequence of certain attractions or properties which the ice and caloric are respectively possessed of. How these substances have such attractions, we are far from pretending to explain. But it is conceived that this theory ought no more to be required to explain the cause of attraction, than other theories, on this, and various chemical subjects, none of which afford any explanation of such difficulties. There is one circumstance in this theory which is not quite easily conceived, though of the utmost importance to the right understanding of the doctrine. This is, that water at  $32^{\circ}$  contains a quantity of heat measured by the number of degrees of its temperature, from

the natural zero, multiplied by the number expressing its specific caloric. This must be very obvious to any person who attends to the terms and conditions of the hypothesis with due care. Yet I have been surprised to observe how many errors of inattention to this has caused: how many arguments have been stated, by men truly ingenious, in opposition to this doctrine, which have appeared wholly ill-grounded, or entirely unintelligible, without a previous knowledge of this misconception.

In this way of considering the matter, both parties agree, that in a general gross view of the subject, caloric is the cause of fluidity, or rather affinity by which caloric enters into the solid. The question is, how does it affect this fluidity? Does it combine with the solid in no definite or measurable quantity? Or does it enter into it, in a quantity proportionable to the new capacity? And, after all discussions and disputes regarding this subject, it will, I think, be found that is the real state of the inquiry, in the case, at least, where the theory is stated as I have now done.

The objection proposed by Dr. Black, on the supposition, that in defence of this theory, it might be said, that the sensible heat is first increased a little, so as suddenly to weaken greatly the cohesive attraction, after which the latent heat enters, does not by any means appear very forcible. Surely no person could make such a supposition, who knew that ice is not expanded, but contracted by fusion.

From which it ought to follow, that the cohesive attraction, far from being weakened, acts more powerfully at the diminished distances, if at least heat were the cause of fluidity solely by its expanding powers. It cannot be maintained, therefore, that caloric is the cause of fluidity, by separating the particles of the solid, to such a distance from each other as to be out of the reach of each other's attraction. But if it were so argued, the cooling of water to  $22^{\circ}$  would afford no additional proof. For, undoubtedly, the lower the temperature of water is reduced, the less chance can cohesive attraction have of causing solidity, since every new degree of cooling is accompanied with a corresponding expansion of the water, according to the best experiments. No doubt, it is a very curious fact, that water can be cooled to a lower degree than  $32^{\circ}$ , and still more extraordinary that the slightest motion immediately is followed by a crystallization. And Dr. Black's question, what hinders the cohesive attraction from producing its effect, might be varied so as to require an answer from the supporters of his own theory. It might be asked, how does it happen that a chemical combination of ice and caloric, which cannot be decomposed, by the confessedly superior attraction for heat of the cold bodies surrounding, can be suddenly, and as it were magically, dissolved by mechanical means, by a slight percussion? It is clear, that the cold substances around do attract caloric with greater force than water does at other times.

For water, surrounded by bodies of the temperature of  $22^{\circ}$ , will, without the greatest care to prevent motion certainly be frozen. And it will be found impossible to melt ice enveloped in substances at  $22^{\circ}$ , by any means. From which it follows, if a chemical union really does take place in this case, that there is here an anomaly in the laws of attraction, which is not easily reconciled with known principles.

The nature and laws of corpuscular affinity are certainly still very imperfectly understood. And there may probably be varieties, in its mode of action, yet to be discovered, sufficient to explain this and many other chemical difficulties. There are various powers, the extent of whose influence is yet almost unknown, and which yet probably interfere in many processes of chemistry. There may also probably be other powers, or substances, and situations, in which already known powers operate. Magnetism, though of old, is not of ancient discovery: electricity is but recently discovered; and Galvanism, even if only a new instance of the electrical fluid, is a proof of the possibility of many agents existing unknown, or in situations altogether unexpected. Something, therefore, ought to be allowed for such circumstances, when we reason concerning the action of particles among each other. And Dr. Black's question, just alluded to, may be answered differently on these grounds. It may be said, that when fluidity takes place, not only an

absorption of caloric is observable, but the specific caloric becomes greater. From arguments already given it appears, that this latter effect is adequate to the production of the former, and that the new heat must enter into the body in a ratio guided by the increase of the specific caloric. The fluidity itself, however, remains to be accounted for, and if it even were granted, which it is not, that heat according to our theory could not be this cause, it would be unwarrantable to say that no cause could exist, while it is recollected how very imperfectly the laws of particular attraction are understood, how little we have ascertained to what degree the influence of electricity is exerted during fusion, or what other powers happier discoveries may yet demonstrate to operate in producing this effect. For it still remains clear that it is impossible to deny that the change of the capacity of the ice must, independently of all other circumstances, be accompanied by an absorption of caloric by the water formed. These arguments, however, only apply to Dr. Black's statement; and it is again to be recollected that none of these suppositions is at all necessary to the truth of the theory in question.

It is asserted that as water above  $32^{\circ}$  is always fluid, so water below  $32^{\circ}$  ought always to be solid; but that this is not so. The reason of this is said to depend on the expansive power of heat. But surely if a slight increase of sensible heat *were* to cause fusion by separating the particles, the cooling of



water below  $32^{\circ}$  need not produce the opposite effect. Ice we know expands by heat, but water as often expands by cold. And if ice at  $32^{\circ}$  by a small increase of sensible heat has the cohesive attraction of its particles destroyed or greatly diminished, it does not by any means follow that water cooling below  $32^{\circ}$  ought to have the cohesive attraction of its particles increased. The cases are directly opposite; and as ice ought to become fluid above  $32^{\circ}$  by the separation of its particles, so water ought to become more liquid every degree that its temperature falls below  $40^{\circ}$ , for there lies the maximum of the contraction of water.

It is not, however, to be considered as a fair statement of the doctrine of capacities, to say that bodies first change their specific heat, and then admit or absorb the latent caloric, the substance being thus left to become fluid in the best way it could. In a general view it may be allowable to grant that heat is the cause of fluidity in the same sense as we say that an alkali is the cause of an oil becoming soap. We see, for example, very clearly that ice cannot bear a temperature higher than  $32^{\circ}$  without fusion; neither can it be fused without heat unless by the help of chemical agents. Caloric, therefore, is certainly requisite for the formation of water. But let us suppose a quantity of caloric to enter a portion of ice at  $32^{\circ}$ ; if there is enough of caloric, the whole ice is converted into water; if there is less than enough, the ice is only converted in part into the fluid form.

The question here is not whether heat is the cause of fluidity, but a more difficult one, how does heat produce it. The theory of capacities asserts that the caloric rushes into the ice enlarging and satisfying its capacity at one and the same instant and by one and the same action. The fusion, the increase of capacity, and the heating of the body according to the new capacity are obviously simultaneous, the work of the same moment. It may be difficult to explain how all this happens ; but not more so than to explain according to any hypothesis how fluidity should be produced by heat.

The increase of the capacity of all solids when fused, and of all fluids when vaporised, is surely a striking fact, which deserves consideration. Yet it has met with very little or indeed with none at all from Dr. Black's explanation. Not only does ice require a quantity of caloric to exist as water, but after it is become water, heat has no longer the same effect on it, but raises its temperature much less ; nay, this continues true below  $32^{\circ}$  Fahr. and as far as we know to the bottom of the thermometric scale. We see in all cases, that whenever the capacity is enlarged either of a simple body or the mean capacity of two bodies combined, that heat or rather temperature disappears.

This is without a single exception, that is known, in the whole history of chemical unions. It is not easy to conceive how ice could possibly have its power of containing heat increased from eight or nine to

ten, without absorbing heat. It is actually proved that it does increase its capacity, and this fact demands explanation and attention as much as the production of fluidity itself. We cannot avoid asking, how can a body augment its capacity without either becoming colder or absorbing heat. And if this be found difficult or impossible to explain, it must necessarily be admitted that the increased capacity is the cause of the influx of heat; or, at least, that the increase of capacity and absorption of heat are co-existent and somehow connected. It seems indeed to me altogether undeniable that, at all events, some part of the caloric of fluidity, as it is called, must go to raise the temperature of the body fused.

In the case of vapour, the same general arguments may be applied. If we suppose the water to be heated to  $212^{\circ}$ , and caloric still to be flowing in, the case of vaporization appears completely similar to that of fusion. Only in the former instance there is no peculiarity in the action of heat with regard to expansion. Steam at  $212^{\circ}$  is greatly more bulky than the water from which it was formed. But if we imagine the temperature of the water to be  $212^{\circ}$ , and no more caloric to be entering, then let the pressure of the atmosphere be suddenly removed, and a quantity of vapour will be instantly produced, and the temperature of the whole will sink considerably. In this case it has been said that the removal of the pressure was the cause first of the increase of the ca-

capacity, and afterwards of the cold produced. But these circumstances occur at one instant, and no certain inference of this kind can be drawn. The phenomenon, however, is not to be forgotten in the consideration of the cause of fusion. One remark further will be admitted, that if the chemical combination of heat be at all allowed, the caloric is as much chemically united with the water at  $212^{\circ}$ , as it is with the vapour, formed by removing the pressure; and the same is true to whatever degree the temperature of the water be raised. Therefore we cannot allow, with the ingenious Dr. Robinson, that pressure has the power of preventing chemical combination in this instance.

According to this theory, therefore, it is held that caloric exists in all bodies in the same state, it matters not whether retained by chemical or other powers. This view is remarkable for its simplicity. All bodies are affected in a similar way by heat. Ice and iron, for example, we know to have different capacities for heat. We see that each thermometrical degree in each is raised by a quantity of caloric proportional to its capacity. This is true as far up and as low down in the scale as it has been possible to observe. We infer that it is true also to the point of total privation of all heat. Even, if it should not be so, I shall afterwards endeavour to shew that the argument is unaffected. Iron and ice are therefore concluded to contain heat in proportion to their capacities. But the same is true of any other bodies

chosen. The like circumstances appear also in ice and water; their capacities are different, that of water being highest. It ought therefore to contain more caloric than ice. But by experiment it actually does. Hence ice and water are to be considered in the same comparative view with regard to heat, as ice and iron, or glass and gold, or any other two bodies having different capacities for heat.

This doctrine supposes nothing of the intimate nature of heat. It agrees equally well with the hypothesis of matter or that of motion. The other mode of explanation, however, seems to require the previous admission of the material nature of caloric, and that it is governed by the usual laws of other matter; a supposition which many philosophers of the present day are by no means disposed to admit.

The advantage of this hypothesis is, that it affords an explanation of all the phenomena of fusion; but the contrary theory, though applicable to other circumstances, does not explain why a body combining with latent heat, should have its specific caloric ever after greatly enlarged. It might as well, for any reason assigned, happen in directly the reverse manner; especially when we consider, that, so far from there being more room between the particles of the fluid than between those of its corresponding solid, there is plainly less in many instances. Either therefore, the increased capacity and the fluidity are the co-existent consequences of some common cause,



of the union between the latent caloric and the solid is accompanied, at the moment of its formation, with a new capacity for heat in proportion to which the latent caloric is great or small. It is in the present state of knowledge no easy task to say, whether any adequate cause, besides caloric, for the production of fluidity is known, or is likely to be discovered: it would be presumptuous to affirm or deny too boldly. I have already alluded to the agency of the electrical fluid, and there can be no doubt that it is somehow implicated in the change from the solid to the fluid state. Ice is well known to be a non-electric. After fusion, however, it is one of the best electrics known, with the exception of the metallic bodies. I do not know whether it has occurred to any other person to try if this property extends any farther. But as it might strengthen the arguments of one or other side, I have thought it an object of curious inquiry, and made a few experiments on this subject. I cannot say, however, that much light has been thrown upon the object of consideration by these trials.

The substances which I thought of examining were four of the non-electrics most easily melted. I mean spermaceti, bees' wax, common rosin, and sulphur. A metal cup filled with a solid mass of each of these was put upon the conductor of a powerful electrical machine, and sparks were attempted to be drawn through them without success. Only when the coat



of the non-electric was very thin, a few sparks forced their way through the spermaceti and the rosin. Each of these substances was submitted to the same trial, but in a fluid form. In the melted wax and spermaceti, when a brass rod was approached to them perpendicularly, a violent internal motion took place sufficient to drive great part of the materials out of the vessel. Some sparks also appeared to proceed from the fluid, but a great number came from the bottom of the vessel, the electrical fluid forcing the melted wax and spermaceti out of its place and leaving part of the bottom of the vessel uncovered. It appears thus that electricity has a much greater tendency to displace these two bodies than to pass directly through their substance. But I am disposed to think that the wax and spermaceti are not so complete non-conductors in their fluid as in their solid state. The melted sulphur and rosin also gave indications of the passage of a small portion of electricity through them more than they did in their state of solidity. Upon the whole, though no very positive inferences can be drawn from these experiments, yet I think that they rather tend to favor the opinion that fluids conduct electricity better than their relative solids. It might be worth while to pursue this subject farther, and to determine the changes sustained during fusion, by various substances with regard to their electrical properties, especially by the more easily fused metals, such as

may be permitted to suspect the change of capacity as the cause of the other change. It may, also be remarked that a theory, which explains all the phenomena excepting this augmentation or diminution of capacity, cannot be considered as equally complete with one which extends to this point, as well as to all others; which are required to be explained.

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## ESSAY III.

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### *ON THE CAPACITIES OF BODIES FOR HEAT.*

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THE comparative elevations of temperature produced in different bodies by the same or similar quantities of caloric, are supposed to depend on a peculiar property of these substances, which has been distinguished by various names. It has been styled by different philosophers, the capacity for heat, the specific heat, the relative and the comparative heat of bodies. These capacities are regular, that is, each degree of temperature is raised in the same body by a like portion of caloric : and in many instances this has been experimentally proved to be true, through a long range of thermometric degrees, provided no change of form takes place. And in general it may be affirmed, that no exception to this regularity or permanence of capacities has been observed in any case whatever, excepting

in the instance just alluded to, when a change of the substance took place from a solid to a fluid, or from a fluid to an aëriform appearance. To compare this property of bodies we may take either equal bulks or equal weights of each. In the case of employing equal weights, indeed, it has been objected that the phrase capacity for heat is only intelligible when equal bulks are taken. This objection does not strike me as very forcible, and it would appear that the capacity of equal weights of two substances may be compared with as much ease and precision as the capacity of equal bulks, and be equally well understood. For it is by no means necessary to restrict the meaning of capacity to mere capaciousness. In common language it is at least as often used to express the power of acquiring or admitting as that of containing. From the other words used to express the same idea by Dr. Black and Dr. Irvine as mentioned by Dr. Robinson, such as affinity for heat, and appetite for heat, it is probable that the meaning of capacity was not intended to be capaciousness alone.

There has been a good deal of discussion regarding the propriety of using the various expressions for the property of which we now treat. The word capacity, in particular, has been objected to, chiefly on the ground of its being a theoretical phrase which supposes a peculiar state of the caloric in bodies; but, unless taken in the sense of capaciousness, it cannot lead to any other inference than all the other words do. Upon the whole, it seems to me at least,

that any of the abovementioned modes of expression may be employed without objection, a clear definition of the meaning attached to it being premised. The word capacity was first employed in this sense by Dr. Black and Dr. Irvine ; the expression, relative heat, by Dr. Irvine, comparative heat by Dr. Crawford, and specific heat, I believe, by Professor Wilcke.

The cause of the various capacities of bodies for heat does not seem to be satisfactorily accounted for. The capacity does not follow the ratio of the specific gravity or of any other property of bodies as far as can be determined. It appears, that every body has a peculiar power of attracting or admitting a quantity of caloric, which is as peculiar to itself as its weight or its chemical qualities. It would certainly tend to simplify the theory of heat very much if any account could be given of this property of bodies, which could be referred to a more general view of the subject.

It has been suggested by the author of an ingenious system of chemistry that bodies unite with caloric from an affinity which they have for it in like manner as all chemical compounds are formed. But the affinity varying in degree, it follows that different bodies should require different proportions of caloric to form a new compound. Bodies, however, conduct caloric with various celerity. The same author attributes the retardation of caloric in its passage through these bodies to the affinity exercised

by the particles of matter near which the caloric passes. From which he concludes that the conducting power of bodies is inversely as their affinity for caloric, or inversely as their capacity for heat. It follows from these statements that the truth of this theory must rest upon the correspondence of the capacities and conducting powers of bodies as determined by actual experiment. Dr. Thomson has accordingly made a few experiments with this view, and he found the number expressing the conducting powers to be more nearly, as he himself says, the inverse of the specific caloric of the bodies as determined in his table than he could have expected in experiments of so delicate a nature. His numbers are as follow :

Bodies.	Specific Caloric	Conducting Power.		Difference.
		By theory.	By experiment.	
Water	1	1	1	0
Mercury	0.31	3.22	1.600	+1.38
Linseed oil	0.528	1.9	1.585	-0.81

If this table were implicitly to be adopted, it would, as far as it goes, be in some degree consonant with the principles of the hypothesis, excepting that the computed conducting power of mercury differs too much from the experimental conclusion, being considerably more than a fourth part less than it should be. The differences, too, are not upon



the same side in both cases ; for if the conducting power of the mercury be one-fourth too small, that of the linseed oil is too great by more than a third. It would have been desirable, that Dr. Thomson should have given a more detailed account of experiments from which he draws so very important inferences. There are many minute particulars which tend very much to illustrate the application of experiments to theories, of which no account can be taken in a table containing merely a numerical result. The specific caloric of mercury is stated considerably higher than it is usual to allow it. This conclusion is drawn, as we are informed from Dr. Thomson's own experiments, and is, as he affirms, nearer the results obtained by others than Dr. Crawford's determination is. As upon the accuracy of the numbers in the above stated table all the proof of the theory in question rests, it is proper to inquire into the reasons of admitting or rejecting the new number for the specific caloric of mercury.

It is certainly just to allow the fairness of Dr. Thomson's statement of his own experiments, though he has not descended to particulars. But I cannot understand upon what principles he has compared his own results with those of others in the case of the specific caloric of mercury, so as to induce him to say that his numbers at all approach those of any other philosopher. In the table of specific heats in that gentleman's system of chemistry, he gives 0.31

as the mean determination of Lavoisier and Kirwan. The experiments of the latter of these philosophers, are contained in Magellan's Treatise on Heat, which I have not been able to consult. But in the *Annales de Chimie*, vol. V. where Lavoisier's experiments on this subject are quoted, his number is given as 0.029 which is very wide indeed of Dr. Thomson's, and would require Mr. Kirwan's to have been no less than 0.591 which is extremely improbable. There must have been some mistake here. Dr. Black\* informs us that the same quantity of heat which raises the temperature of water two degrees raises that of mercury three; therefore, the capacities of these two substances are to each other as 3 : 2. Or water being taken as 1., mercury will be .66. But these are the specific heats of equal bulks, and when equal weights are taken, as must be done to compare the results with Dr. Thomson's, the proportion stands as 1. :: 0.049 nearly. For since equal measures of water and mercury are heated by equal quantities of caloric as 2 : 3, take the specific gravity of mercury at  $13\frac{1}{4}$  in a rough way; the 27 half ounces of mercury gained  $3^{\circ}$ , while two half ounces of water lost  $2^{\circ}$ . But two half ounces of mercury would have gained more in the ratio of 27 : 2, or  $40^{\circ}.5$ . Therefore the specific caloric of water is to that of mercury in the inverse ratio of their augmentations of temperature, as  $40.5 :: 2$ , or as 1 : 0.049. This is certainly somewhat different from Lavoisier's determination, but it is out of all sight of Dr. Thomson's.

\* Black's Lectures, vol. 1, page 80.

Dr. Crawford \* observes that the quantity of heat which raises a pound of water one degree, raises a pound of mercury  $28^{\circ}$ ; or that their specific heats are as  $1 : 0.035$ , equal weights being taken. Dr. Irvine expresses the very same opinion nearly in the same words, and adds that he has repeated the experiment many times. Dr. Crawford has in another part of his work † mentioned that the changes of temperature in these two substances are to each other as  $3 : 2$ , in the same terms as stated by Dr. Black. There seems some inconsistency in these two accounts of the same facts.

It appears then that the experiments of other philosophers have more nearly coincided with each other than with Dr. Thomson's. We have by Lavoisier, by Black, and by Crawford, or Irvine, three numbers given, as below :

Black	0.049
Crawford	0.035
Lavoisier	0.029
Mean	0.037

The mean here is extremely wide of the number in the former table, and it is impossible under these circumstances to give assent to deductions of the cause of the conducting power of bodies founded

\* Essay on Animal Heat, p. 5.

† Page 88, *ibid.*

upon the stated numbers. There may, perhaps, have been some error in considering the comparative bulks instead of the comparative weights of the substance under examination.

But there have been other experiments made for the purpose of ascertaining the conducting power of substances which may perhaps serve to throw further light upon this subject. These have been made by Ingenhouz, Meyer, and others. Some of these are stated below, comparing the capacities for heat with the conducting powers.

Conducting powers in the order of the names.	Capacities.	
	Of equal weights.	Of equal bulks.
Silver.	0.082	0.9201
Gold	0.050,	0.9520
Copper } nearly equal	0.1121	0.9847
Tin }	0.0661	0.4878
Platinum }	0.13 *	
Iron } much infe-	0.1264	0.9955
Steel } rior to the		
Lead } others	0.0424	0.4857
Glass	0.174	0.5793
Charcoal	0.2631	

The capacities ought in this table to be least at the beginning of the table, and to increase downwards, if the capacities and conducting powers of bodies depended on their affinity for caloric. But a slight inspection is sufficient to shew, that no precise inference of the nature of specific heats can be drawn from

\* By my own experiments.

it. The numbers seem almost all irregularly placed. Further experiments upon this subject are, however, highly desirable, and would probably tend considerably to illustrate this obscure part of the theory of heat. But the principles upon which Dr. Thomson wishes to establish this opinion, in which he has in some measure followed Dr. Black and Pictet, are by no means perfectly ascertained. It does not necessarily follow because heat passes with some difficulty through bodies, and is detained among the particles, that it is attracted by them. For this effect may result not only from the attraction of the particles for caloric, but also from their attraction for each other. Nobody attributes the resistance of air to the passage of a bullet through it, to the attraction of the air for the bullet. And in the same manner, if caloric be either a body of which the small parts mutually repel each other, or if it have a disposition to rush into every portion of space where it can find room, it may effect its passage through the interstices of the particles more or less swiftly, in proportion to the disposition of the particles to make way for it.

The numbers expressing the capacities of bodies are determined as has been already mentioned, by observing the rise of temperature produced in each body by a known quantity of caloric. If the same quantity of heat be applied to equal bulks or equal weights of different bodies, the capacities of these bodies are to each other inversely, as the rise of temperature observed in each. Several precautions are necessary to ensure the accuracy of experiments of

this nature. Particular care must be taken that no chemical union take place among the substances mixed for examination. Proper allowances must be made for the heat or cold communicated by the vessel employed, and for the change of temperature produced by the surrounding air. When these circumstances are attended to, large quantities of the materials being used and at various temperatures, the results will probably approach to accuracy. Perfect accuracy can never be attained, but care and skill will remove or lessen almost every obstruction. Water has been generally used as the medium of comparison, its own specific heat being estimated at unity. Where no chemical action is exerted by water on the substance, of which we wish to ascertain the capacity, it is frequently convenient to use it as one ingredient of the mixtures. In general there are three different ways of mixing bodies with each other for this purpose: 1, Equal bulks or equal weights may be taken so as to produce unequal changes of temperature, and then the capacities are inversely as the changes of temperature produced; or 2dly, Unequal quantities may be mixed so as to produce equal changes of temperature, in which case the capacities are inversely as the quantities of matter; or, 3dly, Unequal quantities may produce unequal changes of temperature, and then Dr. Irvine shewed that the capacities are reciprocally as the quantities of matter multiplied into their respective changes of temperature. In all these ways a great degree of accuracy may be attained, and I can



see no foundation for the opinion that equal bulks or weights are preferable to unequal ones; on the contrary, the easiest and most certain way seems to be the third. It is frequently necessary to use a third substance as a medium with which two others may be compared, that their relative capacities may be discovered. This is requisite when two bodies have a chemical action upon each other, as sulphuric acid and water. It would be impossible to discover the specific heat of that acid by mixing it with water in any proportions. A third body must be mixed with each: and in this instance, as will be seen in the second part of these Essays, Dr. Irvine employed pounded glass, adding to a given weight of each a quantity of glass sufficient to change their temperature by the same number of degrees: then the capacities were as the weights of glass added. This method differs from all those already stated, and, in general, when equal quantities of two different bodies are examined by help of equal quantities of a third, the capacities are inversely as the changes of temperature, but if equal changes of temperature be produced by unequal quantities of the third body, then the capacities are directly as the masses added. But if unequal weights or measures be taken, the capacities may easily be found with reference separately to the third body in one of the three methods first stated.

Proceeding in one or other of these ways, Dr. Irvine determined the capacities of a multitude of bodies, chiefly with a view to certain opinions re-

garding the nature of caloric which he held or wished to examine. I am possessed, however, of the results of comparatively few of his experiments. But I have subjoined his numbers expressing the capacities of such bodies as I have been able to ascertain to have been investigated by him, in the following table :

Substance.	Capacity of equal weights.
Water	1.000
Ice	0.600 +
Oil of Turpentine	0.400
Iron	0.143
Crown glass	0.200
Spirit of wine	0.930
Fluid Spermaceti	0.320
Sulphuric acid	0.333

It has been observed above, that bodies in general have their specific caloric increased during their fusion. For the most part, this change is performed at one thermometrical degree and at the same moment. But this is not without exception ; for some bodies pass from the solid to the fluid form not directly, but after undergoing various degrees of softness which at last terminates in perfect fusion. Of this kind we may enumerate bees' wax, sealing-wax, spermaceti, iron, and probably some others. Dr. Irvine made some experiments upon this subject by observing the rate of cooling of a mass of fluid spermaceti or bees' wax. The result of these is as follows :

## EXPERIMENT I.

*On the cooling of melted Spermaceti.*

Hour.	Degree of Heat.	Diff. of Degrees.
12. 10'	128	
11	123	5
12½	120	3
14	118	2
16	118	0
18	118	0
20	118	0
22	117	1
24	116½	½
26	116	½
28	115	1
30	114½	½
32	114	½
33	113	1
34	112½	½
35	112	½
37	111	1
38	110	1
42½	109	1

## EXPERIMENT II.

Hour.	Degree of heat.	Diff. of Degrees.
1. 18'	156	
20	140	16
22	128½	11½
24	122½	6
26	119½	3
28	118	1½
30	118	0
32	118	0
34	117½	½
36	117	½
38	116½	½
40	116	½
42	115	1
44	114½	½
46	113½	½
2. 52	71	42½

ON THE CAPACITIES OF  
EXPERIMENT III.

Hour.	Degree of Heat.	Diff. of Degrees.
3. 7'	160	
9	143	17
11	131	12
13	123	8
15	119½	3½

In this experiment the first congelation began when the temperature was 127°.

EXPERIMENT IV.

Hour.	Degree of Heat.	Diff. of Degrees.
3. 21	157	
23	142	15
25	131	11
27	123	8
29	119½	3½
31	118	½
48	118	0

In this experiment the thermometer was kept in the centre of the fluid. The first congelation began at 126° +, and all was solid, though soft at the last observation.

In all these experiments but the fourth, the thermometer was kept at the edge of the fluid. The temperature of the air was 55°. The quantity of spermaceti used was one ounce.

## EXPERIMENT

*On the Cooling of Melted Bees' Wax.*

Hour.	Degrees of heat.	Diff. of degrees.
12.54'	176	
56	154	22
58	142	12
1. 0	142	0
2	142	0
4	141 $\frac{1}{2}$	1 $\frac{1}{2}$
6	139 $\frac{1}{2}$	1
8	138	1 $\frac{1}{2}$
10	136 $\frac{1}{2}$	1 $\frac{1}{2}$
12	134 $\frac{1}{2}$	2
14	133	1 $\frac{1}{2}$
16	131	2
18	129	2
20	127	2
22	125	2
24	123	2
26	120 $\frac{1}{2}$	2 $\frac{1}{2}$
28	118	2 $\frac{1}{2}$
30	116	2 +
32	113 $\frac{1}{2}$	2 $\frac{1}{2}$ -
34	111	2 $\frac{1}{2}$
36	109	2
38	107	2
40	105	2
42	103 -	2 +
44	100 $\frac{1}{2}$	2 $\frac{1}{2}$
46	98	2 $\frac{1}{2}$
48	96 -	2 $\frac{1}{2}$
50	93 $\frac{1}{2}$	2 $\frac{1}{2}$ -
52	92	1 $\frac{1}{2}$
54	90 $\frac{1}{2}$	1 $\frac{1}{2}$
56	89	1 $\frac{1}{2}$
58	87	2
2. 0	85	2
2	83 $\frac{1}{2}$	1
4	82 $\frac{1}{2}$	1
6	81 $\frac{1}{2}$	1
8	80	1 $\frac{1}{2}$
10	78 $\frac{1}{2}$	1 $\frac{1}{2}$
2. 6	77	21 $\frac{1}{2}$

In this experiment the thermometer was kept at the edge of the fluid. The quantity of wax amounted to one ounce and a drachm. The temperature of the air was  $52\frac{1}{4}$ .

It was from these or similar experiments, that it was concluded by Dr. Black and Dr. Irvine, that these bodies, which soften previous to their complete fusion, take in a part of their latent heat during their softening, and give it out again during their gradual hardening. Dr. Irvine conceived further, in pursuance of the principles of his theory, that these substances change their capacities for heat gradually. It ought to be observed that this forms no exception to the principle that bodies only change their capacity with their form. For we have here a gradual and progressive change of a capacity, at every step of which a corresponding change of form takes place. It is in vain to bring this as an argument against the permanence of capacities. With as much reason it might be objected that solids in general change their capacities during fusion, and fluids when they assume the gaseous form. There is no change of capacity during the softening of bodies of which warning is not given to the senses by an alteration in the appearance of the body approaching to fluidity; and the advocates of the opposite opinion may be challenged to produce any instance of an alteration in the specific caloric of any body which does not accompany a more or less considerable change of form.

The knowledge of the capacities of different sub-



stances is of importance in various points of view, but especially with regard to the quantity of caloric contained in bodies. It is impossible to determine to what uses the tables of specific caloric may be applied in future, but a store of facts always turns to advantage at one time or other. It is probable that if ever philosophers shall arrive at a more distinct knowledge of the actions of the small particles of matter upon each other, an accurate acquaintance with the specific calorics will be of much importance. In the mean time our present consideration must chiefly be directed to capacities as the probable measures of the absolute heat of bodies.

It is to be inquired then, do bodies contain caloric in proportion to their specific heats. This appears to be highly probable, when it is considered that the experiments for determining the specific heats, of bodies are only observations of the changes of temperature, produced in equal quantities of each by equal quantities of heat, and that it appears, that as far up and as low down as these experiments have been made in the thermometric scale, there is no alteration of capacity, while the body retains the same form. For the perfect justness of this observation it is only necessary to refer to the experiments of the best authors on this subject, as for example, Crawford and Wilcke. As far then as actual observation has gone, capacities are permanent, and it is reasonably inferred that the same thing is true to the point of total privation, since of the contrary supposition there is neither experimental nor theoretical evidence. If it

were possible to add experimental proof of this, it ought undoubtedly to be done. But this clearly is and always must be impossible. Since then there is not the smallest assignable reason why the capacities of bodies should vary in extreme colds, but exceedingly plausible arguments why they should be permanent; let us for a moment consider what consequences ought to follow the one or other of these suppositions, and compare them with the actual state of facts. It is evident, that if capacities are permanent, two bodies which diminish their capacities when mixed, ought always to become warmer than they were before mixture. And this is by experiment found to be the case without a single exception. On the other hand, bodies which become warmer by mixture ought, if capacities are permanent, or, in other words, if bodies contain caloric according to their capacities, always to have the capacity of the mixture less than the mean of the capacities before mixture. And this is no less certainly and universally true than the former. These coincidences are surely very remarkable, and ought to make us pause to consider whether this accompanying change of capacity is competent to account for the changes in the situation of the caloric of the bodies. But since it is competent to this purpose, it is unnecessary to make another supposition to account for the phenomena. For no theory can possibly deny that the change of capacity is more or less concerned with the changes of temperature. And it is contrary to the maxims of sound philosophy to

make more suppositions than are sufficient to explain the facts.

We are acquainted with three states in which most, if not all bodies may exist with regard to heat, the solid, the fluid, and the aëriform state. It has been supposed, and is not wholly impossible, that there may be some other state at very low temperatures. It may also be supposed, for it cannot be demonstrated, that capacities may vary in extreme colds. Of the first of these suppositions it is needless here to say any thing, as it is equally out of our power to prove and disprove such an hypothesis, and allowing it to be true it would affect the reasoning concerning the absolute heat of bodies, only in as far as a change of capacity accompanied a change of form. The consideration therefore, of the effect of a change of capacity on the computation of the quantity of heat contained in a substance, will include all cases. It has been generally supposed that bodies cannot contain heat in proportion to their capacities, unless the capacities be permanent. M. Seguin has positively asserted, that this preliminary is necessary to be admitted if this doctrine be true, and the assertion has passed current. As far as I perceive, however, such a change will make no alteration whatever on the absolute heat of any body computed before the change of capacity takes place. At least whatever differences may arise, no new argument can be derived from that source, further than from the already agitated point of dispute,

the change of capacity during the process of fusion.

The proposition to be defended is, that bodies contain heat in proportion to their specific caloric. If the capacity remain permanent to the natural zero or point of total privation of all heat, it appears that an equal portion of caloric will be required to raise the temperature of the body each degree to the melting point. Consequently, if the natural zero be the same point in all bodies, any two solid substances at the same degree of temperature contain heat proportionally to their capacities. For the number of degrees is the same in both, and the whole number of the degrees in any one is equal to the whole in any other, and each degree in both bodies is raised by a portion of caloric, proportional to the capacity of each. Whence the whole heat in the one body, is to the whole heat in the other body, as the capacity of the one is to the capacity of the other, if the capacities continue permanent, and all experiments concur to prove this to be the case, as far as it has been possible to make them.

If however the capacity be supposed to vary, it must either diminish or increase. First let it diminish, then the heat of the body in its state of diminished capacity, plus the heat given out during the whole change, is equal to the whole heat of the body before the change. The body in each state contains heat proportionally to its capacity, and the quantity given out is the difference of the absolute heats in these two cases. Every degree of the tem-

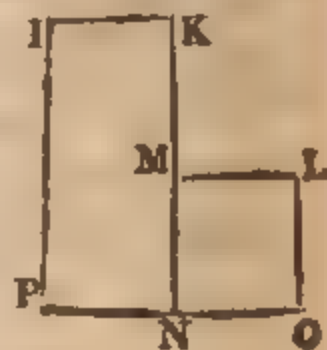
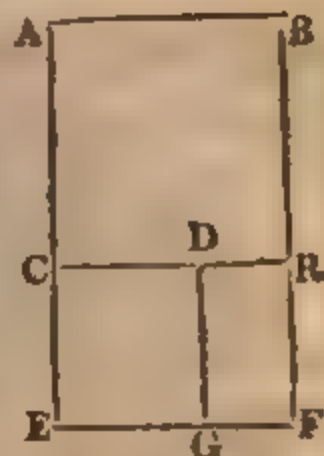
perature of the body in both states is raised by a quantity of caloric proportional to the capacity. Whence, if the higher capacity of the body be known, the absolute heat will be in a known ratio to it, and will not be affected by any diminution of capacity between the melting point and the point of total privation. The object is to ascertain the whole heat in the body as it actually is, not what it may lose if reduced to a lower temperature.

But if the capacity be supposed to increase, the very same arguments apply as in the former case, and it is of no importance what heat may flow into the body at low degrees of temperature, since none of that heat is now in the body, and we wish to know the actual state of the substance with regard to caloric, and not any increments which in certain circumstances it might receive, but by which it is not at present affected.

Though this reasoning is applied only to one change of capacity, it may be equally extended to any number of alterations, and that whether the changes be all diminutions, all enlargements, or any mixture of both. This is sufficiently evident without any further demonstration.

Geometrical figures cannot in cases of this kind be admitted as proofs on either side of the question. But as they sometimes tend to illustrate and render more perspicuous a subject in itself rather obscure, by presenting the evidence more directly to the senses, I shall now repeat the reasoning above stated with the help of a diagram.

Let there be any body whose capacity is represented by the line AB, and the number of degrees in which from the point of total privation of heat is represented by the line AE at right angles to AB. Complete the parallelogram AEFB. It is affirmed that that parallelogram represents the whole heat in the body computed from the point of thermometric elevation A to the natural zero E. This is obviously true, if it be allowed, as every experiment renders probable, that the line AB is equal to the line EF, and that if any line be drawn parallel to AB, and cutting the lines AE and BF, the part intercepted by these lines is equal to AB. But if this is not granted, then such a line must either be less or greater than AB. First let it be found by experiment to be less, as for example CD. Produce CD to R. Then the absolute heat of the body at the temperature C is CG, and the absolute heat at the point A is composed of the figures AR + CG + RG, which last figure is the heat given out during the supposed change of capacity. In the figure





takes place. The point to be determined in this case is the amount or value of the figure  $IPNK$ , and it is clear that this may be found without any reference to the additional figure  $MNOL$  which does not exist at the temperature  $I$ .

It must be understood that I offer this as an illustration only, or at most a different mode of stating the argument, and by no means as a mathematical demonstration, of which the subject is not susceptible. Enough however has been shewn, if I judge truly, to make it clear that M. Seguin's opinion and that of some later authors, who have borrowed it from him, is not well founded, and that whether the doctrine of capacities be true or false, this objection at least, has no weight. The objection is entirely theoretical without any experimental support, and my view here has been to shew that the theory fairly understood is not obliged to presuppose any permanency of the specific heat of bodies.

From these arguments, I hope it will appear, that the only point which admits of dispute is, whether bodies in general give out or take in caloric, during a change of form in such quantity as to make the new absolute caloric proportional to the new capacity. To determine this by irrefragable evidence, it would be necessary previously to know the absolute heat of the body examined, our knowledge of which point will be discussed in a succeeding essay on that subject. It is clear, that to confute the proposed doctrine, would require the same procedure. But,

as the precise amount of the whole heat of bodies cannot yet be said to be accurately ascertained, we must be contented with considering the other parts of the argument, and it is from such considerations that we have concluded that bodies do really contain heat in proportion to their capacities. It has been remarked, that as far as actual experiment has gone, caloric enters into substances in a quantity determinable by their specific heats, excepting only when a change of form ensues. It has been supposed, however, by some philosophers, that as various chemical agents combine with each other only in definite quantities, something of this sort may take place in the relations of heat with matter, and that the lower degrees of temperature may be raised by smaller additions of caloric. The illustration chosen in this instance, that of the union of oxygen with metals and other combustible substances, does not seem at all likely to strengthen the argument. Oxygen and many bodies unite with other substances, only in three or four different doses or portions. But surely this does not bear the remotest analogy to the case of caloric, which combines with or enters into bodies in any imaginable quantity without a known limit. If an illustration must be chosen from the combinations of bodies by chemical attraction, let one be taken which shall analogise in all points. It seems more reasonable to have recourse, for the sake of comparison, to such an union as that of sulphuric acid with water, which can be effected with any propor-

tions of the two ingredients. And in this instance, it does not appear that there is any decreasing or increasing ratio of the quantities of the acid necessary to form a series of combinations with the water, nor is there any reason to suppose that the original, or first unions of the acid with the water, contains less or more acid than the succeeding ones. Nothing can be more theoretical, than the supposition of the necessity of the new unions of caloric being constantly formed, not by the original particle, but by the new compound. Nor can we admit any evidence from such sources, to stand against the more simple deductions from facts. It does not signify to us so much, to know how the union is formed, as to learn the ratio in which the ingredients enter.

In considering therefore, the effects of a change of capacity upon the temperature of any body, it appears necessary in the present state of knowledge to allow, that such temperature must be increased with the diminution and diminished with the increase of the capacity; and in all cases where a change of capacity and a change of temperature occur at the same instant, that certainly a part, and most probably the whole of the heat appearing or disappearing is to be accounted for on the principles here endeavoured to be established. The very idea of cause and effect is closely connected with simple consequence, and has even by some ingenious philosophers been supposed to be only a general observation of the constant appearance of two circumstances

in succession to each other. But in all the phenomena of nature, regarding caloric, there is scarcely an instance where heat is produced or temperature raised without a corresponding change of capacity. We except alone, the production of heat by the sun's rays and the caloric collected by friction. Even these may, perhaps, be referable to the same head when duly examined, and they are but a small part of the objects of discussion, and do not bear peculiarly upon our argument. But, to what is this almost universal coincidence of the change of capacity and the change of temperature to be attributed? Ought we not to conclude, that the one of these must, in some way, be the cause of the other, and since the change of temperature cannot be made to account for the change of capacity, that the change of capacity which affords an adequate explanation of the alteration of temperature, is the cause of that alteration.

M. M. Lavoisier and Laplace have entered into some discussion regarding the manner in which caloric is combined with bodies. They suppose that a body can contain heat according to its capacity, only in as far as that heat is admitted among the particles of the body, but by no means, if the caloric enter into the molecules themselves. The discussion of topics so obscure as the action of particles upon each other, is not often very satisfactory, and it appears totally impossible to ascertain whether caloric does or does not enter into the molecules. It ought first to be ascertained what a molecule is to be considered to be,

whether an assemblage of smaller molecules, or an indivisible particle. In the former case, the molecule may be penetrated by caloric according to the same laws as larger masses are. In the latter, there seems no reason to allow it to be penetrable at all. Luckily it is of no great importance which of these is true, and the subject may be more beneficially considered without adverting at all to the action of the ultimate particles of matter.

Another gentleman, Dr. Thomson, of Edinburgh, has objected to the theory of capacities upon other grounds. I had occasion to take notice of certain opinions of his upon this subject in another place.\* These opinions were originally inserted in the supplement to the *Encyclopædia Britannica*. They were a second time brought forward by that gentleman, in his *System of Chemistry*; I quote his own words.†

“An objection to this explanation of fluidity (Dr. Black’s) was started by Dr. Irvine of Glasgow. According to him, the phenomena may be explained without having recourse to the caloric of fluidity at all. For the specific caloric of water being greater than that of ice, a greater quantity of caloric is necessary to raise water to a given temperature than is necessary to raise ice to the same temperature.

\* Nicholson’s Journal, Vol. 5th.

† Thomson’s *System of Chemistry*, Vol. 1, p. 326, First Edition.



The instant, therefore, that ice is converted into water, its temperature would sink considerably if it did not absorb an additional dose of caloric. The caloric, therefore, which is absorbed during the melting of ice does not combine with the ice and convert it into water, but after the ice has been melted it is absorbed by the water generated. This theory was zealously adopted by Dr. Crawford.

“ Dr. Black observed very justly, that it does not account for the production of fluidity at all. The specific caloric of water is, indeed, greater than that of ice; but how is ice converted into water? This is an objection, which the advocates of Dr. Irvine's or Dr. Crawford's theory (as it has been improperly called) will not easily answer. But independently of this, the theory cannot be admitted, because it proceeds on mistaken notions respecting specific caloric; because it is inconsistent with the phenomena which it pretends to explain, and because it leads to absurd and contradictory consequences.

“ Let us suppose, according to the experiments of these philosophers themselves, that the specific caloric of ice is to that of water as 9:10, and that we mix together equal quantities of ice at 32, and water at 172, the temperature after mixture, supposing the ice not to melt, ought to be 106°; because, for every 9° which the water lost, the ice would receive 10°. But as the ice melts, its specific caloric will become equal to that of water, that is to say, it will absorb  $\frac{1}{10}$  more of caloric than we have supposed. We



must, therefore, diminish the heat of the mixture, by one half of this, or  $5^{\circ}$ ; consequently, according to Dr. Irvine's theory, the temperature of the mixture ought to be about  $100^{\circ}$ ; but this is contrary to fact, for it is only  $32^{\circ}$ . The theory, therefore, cannot be true. But farther, since equal quantities of ice at  $32$  and water at  $172$  after mixture, are only of the temperature of  $32^{\circ}$ , it follows, if Dr. Irvine's theory be true, that the specific caloric of ice is  $140$ , and that of water  $0$ , or at least that the specific caloric of ice is infinitely greater than that of water; for the water is cooled down  $140^{\circ}$ , and the ice is heated  $0^{\circ}$ ; consequently the caloric, which is sufficient to heat water  $140$ , is not sufficient to produce any sensible effect upon ice, or rather melted ice, that is to say, water. This consequence is so palpably absurd and contradictory, that it is sufficient to overturn any theory from which it follows."

This statement differs in no respect, but in greater length and more complete misrepresentation of the subject, from that of which I endeavoured to shew the fallacy in the 5th volume of Mr. Nicholson's Journal. If I do not mistake, it would be sufficient to allow these arguments to be their own answer. But to remove all doubt, as far as I can, I will now proceed to point out the objectionable parts of Dr. Thomson's reasoning.

Dr Thomson begins by representing the theory in question, as if the ice first had its capacity enlarged in the ratio of  $9 : 10$ , and then took in the latent heat, a statement neither necessary nor fair, as I

have elsewhere attempted to prove. The change is instantaneous in each particle, and whether the increase of capacity be the cause of the fluidity or the reverse, or if some other and unknown cause operate, it is impossible to decide. But there is no reason for objecting to the theory, that the enlargement of capacity takes place before the entrance of the caloric. They may be considered as simultaneous. If on the one hand, the cause of fusion is demanded, on the other, stands the increase of specific heat, as remarkable, and equally in need of explanation.

In the third paragraph, where Dr. Thomson employs so much mathematical reasoning to divide  $140^{\circ}$  of caloric, between ice and water, he conceives the ice to begin the enlargement of its specific heat at  $32^{\circ}$ , though Dr. Irvine, and all the philosophers who have followed his doctrines, as far as I know, computed this increase to begin at the natural zero.

There is little wonder that the numbers did not agree in this way of reckoning. If the change be made from  $32^{\circ}$  to the natural zero, the matter becomes plain. Calling the point of total privation of all heat  $1260^{\circ}$  below  $32^{\circ}$  as measured by the capacity of water, or  $1400^{\circ}$  by that of ice, the capacities of ice and water being as 9 : 10; and the caloric of fluidity being  $140^{\circ}$  by the capacity of water, or  $155^{\circ}.555$  nearly by that of ice, let equal quantities of ice at  $32^{\circ}$  and water at  $172^{\circ}$  be mixed, the temperature is  $32^{\circ}$ , and the whole is become fluid. But where are the  $155^{\circ}.555$  lost by the water? They

are gone to make up the difference between each degree of the ice from  $1400^{\circ}$  below  $32^{\circ}$  up to  $32^{\circ}$  and each degree of the water between the same points. That is to say,  $155^{\circ}.555$  are equal to the difference between  $1400^{\circ}$ , measured by the capacity of water, and  $1400^{\circ}$ , measured by that of ice. Therefore, from  $1400^{\circ}$ , by the capacity of water, take  $155^{\circ}.555$  by that of ice, or what is the same thing,  $140^{\circ}$  by that of water, and there remains  $1260^{\circ}$  measured by the capacity of water, equal to  $1400^{\circ}$ , measured by the capacity of ice. From which it appears, that the numbers agree, and the proportions are in perfect harmony. I may here remark, that it is a great advantage to express the absolute heat of the solid in degrees measured by the capacity of the solid, and that however accurately the absolute heats of bodies be determined, the natural zeros can never coincide, unless the numbers are governed by the capacity of the body in question, which circumstance is occasionally overlooked by authors.

Dr. Thomson observes, that when ice melts, its specific caloric will become equal to that of water; that is, it will absorb  $\frac{1}{10}$  more of caloric than we have supposed, and in this assertion lies his greatest mistake. He supposes the absorption of caloric in connexion with the new capacity to begin only at  $32$ , and therefore his argument does not apply to the theory of capacities, but to an imaginary hypothesis of his own. He is further wrong in asserting that ice absorbs  $\frac{1}{10}$  more heat on its fusion.

It absorbs  $\frac{1}{7}$ ; as for example, if ice contain  $1260^{\circ}$  at  $32$ , in melting, it takes in  $140$ , which is  $\frac{1}{7}$  of  $1260^{\circ}$ , not  $\frac{1}{10}$ , and so universally. Surely if Dr. Thomson's statement were right, his mixture of ice and water would not have a different temperature from one of two portions of water at  $32^{\circ}$  and  $172^{\circ}$ , that is a mean. What is meant by diminishing the heat of the mixture by one half of  $\frac{1}{10}$  or  $5^{\circ}$ , I am at a loss to comprehend.

It can hardly be necessary to follow this gentleman through the rest of his argument. Yet I may ask, who knows not that experiments for determining specific heats must not be made on bodies while they change their capacities. Notwithstanding which, Dr. Thomson finishes by inferring, that the capacity of ice is greater than that of water, because  $140^{\circ}$  from water have not heated ice at  $32^{\circ}$  at all, but only melted it: and this he asserts in the very face of a change of specific heat from  $9$  to  $10$ .

To conclude, this is called a consequence so palpably absurd and contradictory, that it is sufficient to overturn any theory from which it flows.

Since writing the foregoing remarks, another edition of Dr. Thomson's work has appeared, in which, though what I consider as the principal errors concerning Dr. Irvine's theory are retained, some points are a little altered. The same gentleman also, in his answer to the Edinburgh Review of his work, has adverted to my former strictures on his Essay on Chemistry in the supplement to the Encyclopæ-

dia Britannica, - He there observes, that he is far enough from thinking that I have established my father's opinions. I hope I am not so thoroughly blinded by self-love, as to imagine even for a moment, that I have been so fortunate as to succeed in that attempt. But I can with equal truth assure my ingenious opponent, that I am just as far from thinking that he has confuted them. Which of us, or whether either of us is right, must be left to others to decide. I am not insensible that many important objections have been proposed from quarters of great authority to my father's doctrines. It has in these Essays been my desire, however inadequate my power, to shew the irrelevancy of these arguments. Dr. Thomson has, with peculiar care and considerable fidelity, detailed all or the greater part of these objections. But I confess, perhaps it is my misfortune, that I cannot see any force in the additional arguments which he has invented upon this subject. These have always appeared to me to proceed upon a misconception of the theory in dispute.

The matter of scientific works is so much more important than the manner, that it is scarcely worth while to advert to the latter. Yet Dr. Thomson could not be surprised that I should not be altogether gratified with the manner in which he accustomed himself at first to discuss my father's opinions, if he reflected on the air of sagacious superiority, the appearance of easily clearing away difficulties,

which had puzzled and do still perplex the brains of less fortunate enquirers, with which that part of his work was composed. I am persuaded Dr. Thomson did not intend to produce such an effect, and am ready to admit that these observations do not apply to the last edition of his generally able performance.

It is with some regret I observe, that that gentleman has accused me of want of candour, in criticising his article in the *Encyclopædia Britannica*, rather than his system of Chemistry. This charge it becomes me to repel, and I trust I shall not attempt to do so without success. The state of the fact was, that it so happened, that I was in possession of the first but not of the last of these publications, and I was satisfied with consulting the System to ascertain that no improvements had been made. I then imagined that I had observed that the arguments were nearly, or at any rate, materially the same in both, and that of consequence, it mattered very little which I endeavoured to answer. Even yet, upon referring to the works, I can perceive no essential difference, excepting that it is no longer asserted, that the specific caloric of iron is greater than that of water. As to the other points, I have in this Essay already attempted to shew, that I might just as easily have replied to the arguments in the System of Chemistry, as to those in the *Encyclopædia*. Though, after all, I do not see any right a man has to interpose the shield of a new work, to defend the frailer body of another performance, the imperfections of which, if even he has



abandoned, he has not retracted. At all events, I hope I have said enough to shew, that I was not compelled to have recourse to the most miserable subterfuge of attacking an old book, because I was unable to answer a newer and more perfect edition.

Dr. Thomson has withdrawn from the second impression of his System many of the arguments which have been here stated: they are not to be found at least in his chapter upon Specific Caloric. But that he still retains the very same views, is obvious from his observations upon absolute heat. I have, therefore, suffered my reply to his arguments to retain its place in this Essay.

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1. The first step in the process of creating a new product is to identify a market need. This involves conducting market research to understand the preferences and behaviors of potential customers. Once a need is identified, the next step is to develop a concept that addresses this need. This concept should be unique and offer a clear value proposition to the target market.

2. After developing a concept, the next step is to create a prototype. This allows the company to test the feasibility of the product and gather feedback from potential users. The prototype should be functional enough to demonstrate the core features of the product. Based on the feedback received, the company can then refine the product design and make necessary adjustments.

3. Once the product design is finalized, the next step is to develop a business plan. This plan should outline the company's financial goals, marketing strategy, and operational requirements. It is essential to have a clear understanding of the costs involved in production and distribution, as well as the potential revenue streams. The business plan will serve as a roadmap for the company's growth and help secure funding from investors or lenders.

4. The final step in the process is to launch the product into the market. This involves implementing the marketing strategy outlined in the business plan. The company should focus on reaching its target audience through various channels, such as social media, email marketing, and direct sales. Monitoring the product's performance in the market is crucial to ensure it meets the needs of customers and achieves the desired financial goals.

## ESSAY IV.

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### *ON THE LOWEST DEGREE OF HEAT.*

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It is probable, that at first sight, there must formerly have appeared little difficulty in ascertaining the lowest degree of heat. People must naturally have imagined, that when water was frozen it no longer contained heat. But this idea could continue only till the observation of the progressive fall of the mercurial thermometer, far below the freezing point of water, and at last of the actual solidification of the quicksilver itself. At what point then are bodies wholly deprived of caloric? Boerhaave, at one period, imagined that the natural zero was at the degree of cold produced by a mixture of snow and salt, which he found to be at the zero of Fahrenheit's scale, though there has since been produced a greater cold by the same mixture. Fahrenheit him-

self, however, by means of nitrous acid and snow mixed together, diminished the temperature to  $40^{\circ}$  below his own zero, and this was boldly asserted to be a degree of cold, such that, if it occurred in nature, would destroy all vegetable and animal life. It is now well known, however, that mercury freezes at this point, and the solidification of that fluid has been repeatedly effected, by the intense colds of Siberia and Hudson's Bay. Almost every period of modern philosophy has fixed a point for the privation of all heat, which succeeding observations have removed lower down.

For want of knowing the ultimate limits of heat and cold, we are unable to determine the geometrical proportions of the real or absolute quantity of heat in one body to that in another. All that we know is, their arithmetical difference. Our knowledge of caloric, therefore, is exactly similar to what we have of a chain of which the extremities are hid from us. We can tell, that such a link is three times as far from one as it is from the other: but whether it is at the middle, or a quarter of the length from the one end, we know not. When Sir Isaac Newton says, that red hot iron is three or four times hotter than boiling water, and that boiling water is three times hotter than the temperature of our skin, I reckon the lowest degree of heat to be when water begins to freeze. And if this be granted, the expansions of the thermometer are in these proportions each other. But the absolute or real quantities

heat in these bodies do not differ so much as by this way of reckoning they have been computed. If we suppose the lowest degree of heat to be when air loses its elasticity, which Amonton imagined to be, when utterly deprived of heat, and which, according to him, would be at  $-400$ , then the heat of boiling water will be to that of the human body as  $400 + 212$  to  $400 + 97$ , or as 6 to 5, nearly. None, before such an examination, would have imagined, that the absolute heat in boiling water, which destroys vegetable and animal life, was to the absolute heat in the human body, which is perfectly mild and temperate, only as 6 to 5; and we shall find reason to suspect that their difference is yet smaller.

It has been already stated in the foregoing essays, that Dr. Irvine took a peculiar view of the properties of caloric. The circumstances of this view I have endeavoured to explain above. In consequence of his theory, he was led to the invention of a method of investigating the absolute heat of bodies, or, what is the same thing, of ascertaining the natural zero, or point of total privation of heat. This method was founded upon the consideration of the change of the capacity of bodies, during their fusion, and of the quantity of caloric necessary to produce fluidity, called by Dr. Black latent heat. I am in this instance again enabled to quote Dr. Irvine's own language, for the explanation of this subject. This is a continuation of the passage already quoted in Essay II. page 59.

"The number of degrees by which a body becomes hotter or colder by changing its form, will be proportional to the quantity of heat in it at the time this change happens, supposing the capacity in a solid form to be, to the capacity in a fluid form, the same in all bodies. Thus, in the instance already given, the quantity of heat  $x$ , which applied to the solid produced expansion 20, in the same body, in a fluid state, produced expansion 10. Here the capacity of the solid is to that of the fluid as 10 to 20, or as 1 to 2, the highest number belonging to the one heated with the greatest difficulty. In this case, whatever be the sensible heat of the body when solid, one half of it will disappear upon its becoming instantly fluid, and *vice versa*. If the absolute quantity denoted by  $x$  produce a degree of sensible heat = 20 in the solid body, in the fluid it will produce but 10. If  $x$  produce 200 in the solid, in the fluid it will produce 100, &c. Or, in the first case, a quantity of heat = 10 will, according to Dr. Black, have become latent, and have lost the property of expanding the liquor in the thermometer, in the second 100, in the third 1000, &c. On the contrary, if the quantity of heat  $x$  in the fluid produced expansion 10, upon change of form it will produce 20, if 100, it will produce 200, if 1000, 2000. Or a quantity of heat = 10, or 100, or 1000 will change from latent to sensible heat, and recover the property of expanding the mercury of the thermometer. Or if



the change be gradual, the body, when freezing, or passing from a fluid to a solid state, will, at its freezing point, preserve itself as long a time warmer, than the surrounding medium, as 10, 100, or 1000 degrees of heat thrown into it would have enabled it to do, without any change of form, and *vice versa* when the body is melting, or passing from a solid to a fluid state, it will preserve itself colder as long as 10, 100, or 1000 degrees of heat taken from it would have enabled it to do."

"The number of degrees by which a body becomes hotter or colder, upon changing its form, will be as the capacity of the solid to that of the fluid, supposing the quantity of heat  $x$  in them, to be the same in all, or to produce the same expansion when the change takes place, or their freezing point to happen at the same degree of sensible heat. Let the quantity of heat in them be expressed by 100. If the capacity of the solid be to that of the fluid, as in the last instance, as 1 : 2, then there must be twice as much heat in the body when fluid as there was in it when cold, or 100 degrees of heat must have become latent. If the capacities be as 1 : 3, then the whole heat, when the body is fluid, must be 300, and the latent heat 200. If the capacities be as 10 : 11, then the quantity 100 must be increased in that proportion, and the whole heat, when fluid, will be 110, and the latent heat 100. And universally the whole heat in the solid being 100, the latent heat in the fluid will be equal to the difference of

the capacities of the solid and fluid, divided by the number expressing the capacity of the solid, and multiplied by the whole heat of the solid. Thus when the capacities are as 1 : 2, their difference is 1, which divided by 1 quotes  $\frac{1}{1} = 1$ , and this multiplied by 100 the whole heat of the solid gives a product of 100 for the latent heat. Here the latent heat is equal to the absolute heat of the solid. When the capacities are as 10 : 11, then the difference 1 divided by 10, the capacity of the solid quotes  $\frac{1}{10}$ , which multiplied by 100, the whole heat of the solid at the melting point, gives 10 for the latent heat in this instance."

"Hence if the quantity of heat  $x$  be different in different bodies, this proportion will still express what the latent heat is. Thus in the last case, the latent heat will be  $\frac{1}{10}$  of the whole heat in the solid, at whatever point the body melts. And hence also, if the melting point alter in water, the latent heat will alter likewise, and be greater if that point be higher, and less if lower; and so in spermaceti, wax, tin, &c."

This extract cannot be extended farther. With regard to the last point, some explanation may be necessary. It is stated that the latent heat ought to diminish with the lowering of the melting point of any body. A consideration of the principles of the theory will readily shew, that every thing else remaining as before, the latent heat must diminish with such a depression of the melting point, otherwise the quantity of the absolute heat in the body could

not bear the just proportion to its capacity. For example, the latent heat of ice being  $140^{\circ}$ , if that substance could be made to melt at a point below  $32^{\circ}$ , the latent heat would be expressed by a number less than  $140^{\circ}$ . If the melting point of ice could be made higher than  $32^{\circ}$ , the quantity of latent heat in the water produced would be greater than  $140^{\circ}$ . The same observations apply equally to the process of vaporization, in which case, if the boiling point be altered, the latent heat of the steam ought to vary in the same way. But it is clear, that instead of this alteration of the latent heat, another in the capacities might take place, either sufficient to counterbalance that, or even to alter the latent heat in a manner directly the reverse. In effect, something of this kind certainly takes place in the instance of steam. Water boils, it is well known, at a much lower point in vacuo, than under the pressure of the air. Mr. Watt found the latent heat of steam in these circumstances to be greater. To rectify the proportions, the capacity of this steam ought to be greater than usual, or that of water less. There is reason to suppose that the steam found in vacuo has really a greater capacity than common steam. At least, we generally, if not universally find, that expansion not directly or simply produced by the operation of caloric, is accompanied by an increase of capacity. How the water is affected by diminution of pressure is not so evident. But from some rude experiments which I made some time ago in the air-

pump, there seemed to me to be ground to suppose that the capacity of water even is a little increased in these circumstances. My experiments were made with a view to determine whether any relation subsisted between the expansion of water in the air pump, as observed by Mr. Canton, and an alteration of the capacity of the water for heat. If the capacity is increased, the expansion is rated too low by Mr. Canton. But if unexpectedly it had appeared that the capacity was diminished, a totally new theory of the expansion of water in the air-pump would arise, since the increase of temperature would be sufficient to account for that of bulk. With this view, therefore, I repeated Mr Canton's experiments, with the addition of a thermometer placed in the water. But cold is uniformly produced, partly no doubt from evaporation, which proceeds rapidly in this case, yet probably in part also from an increase of capacity. That this is so, I inferred from the sudden though small rise of the thermometer immersed in the water, when the air was re-admitted. For it does not appear, that any instantaneous change of temperature could take place from the alteration of the temperature of the air alone.

What has been attempted to be explained in the last paragraph, may perhaps be made more distinct by help of numbers. If the capacities of ice and water be 9 and 10, the absolute heat of ice  $1260^{\circ}$ , and the latent heat  $140^{\circ}$ , it is evident that when ice cools below  $32^{\circ}$  by any number of degrees, the for

instance, that its absolute heat becomes less, and is only  $1250^{\circ}$ . But the absolute heats of bodies are as their capacities. Therefore, since 1250 is not to 1390 as 9 is to 10, either the absolute heats or the capacities must be changed to render the proportions just. If the absolute heat of ice melting at  $22^{\circ}$  be 1250, the absolute heat of water at  $22^{\circ}$  would be one-ninth part greater, or 1388.88, and consequently the latent heat would be equal to the difference between 1250 and 1388.88, or 138.88. But if  $1400^{\circ}$  continued to be the latent heat, then the capacities must necessarily vary, and be as 1250 to 1390, or as 125 to 139.

The latent heat of any body, and the capacity of its solid and fluid forms being given, the quantity of caloric in the body in a solid form may be found. For suppose the capacities to be in the same proportion in all bodies, that of 1 : 2. then as the latent heat increases, the absolute heat increases also; for in this case, the latent heat is always equal to the quantity of caloric in the solid body. But if the latent heat be the same in all bodies, the quantity of the absolute heat in the solid varies with the capacities. Thus if the capacities be as 1 to 2, the latent heat equals the sensible heat as before; if as 1 to 3, the whole heat equals one half of the latent heat; if as 10 to 11, the whole heat equals ten times the latent heat. And universally, the whole heat will be equal to the capacity of the solid multiplied by the latent heat, and divided by the difference of the capacities.

Or, in other words, the difference of the numbers expressing the capacities in the solid and fluid form, is to the number expressing the capacity of the solid, as the latent heat is to the absolute heat of the solid at the melting point. I observe the last of these terms has been erroneously stated by Dr. Thomson to be the whole heat of the fluid.

This problem is the same, as when the ratio of two numbers and their difference being given, it is demanded to find the numbers. These investigations may be expressed by means of algebraical symbols. Let  $x$  = the absolute heat of the solid body expressed in degrees: Let  $a$  and  $b$  represent the capacities of the fluid and solid forms, and  $l$  = the latent heat.

Then it was found that  $l = \frac{xa - xb}{b}$ ; or making  $a$

$-b = c$ , then  $l = \frac{cx}{b}$ , and  $x = \frac{bl}{c}$ . Or in num-

bers, if we suppose the capacity of ice to that of water to be as 9 to 10, and the latent heat to be 140,

then  $\frac{1 \times 140}{1} =$  the absolute heat in ice at  $32 =$

1260: if we suppose the capacities to be as 8 to 9, the absolute heat will be  $= 1120$ ; if as 7 to 8, then 980, if as 6 to 7, then 840. The aid of geometry and of different algebraical processes might be readily called in, to give a different form to these propositions. It does not appear to me, however, that by these means any additional evidence would be acquired, or the matter set in a plainer or more dis-



tinct light than it already is. I shall therefore content myself with the illustrations now afforded, and proceed to consider the other points of this doctrine.

The investigation of the natural zero now proposed, is derived entirely from the consideration of the heat absorbed or set free, during the melting or freezing of solid or fluid bodies, compared with the different capacities of the same body in its different forms. But a knowledge of the same point may be attained by the comparison of the capacities of any two bodies, which unite chemically, before and after mixture, combined with the observation of the heat given out at the same time. This question, however, is fully discussed in another essay.

To any point near the natural zero, it is probable that no body can ever be cooled. The greatest colds observed in nature, or produced by the operations of art, can by no arguments be shewn to approach to the point of total privation of heat. Ether and the gaseous bodies themselves would, long before arriving at such a degree of cold, become solid and coherent masses, deprived of almost every property by which we are accustomed to recognize them. If it be indeed true that bodies imbibe heat by attraction, they never can be cooled to this point by any known means, or if they could for a moment be conceived to be totally deprived of heat, they could never be heated again, since they could not have been

cooled to this point by any ordinary means, till they had lost their attraction for caloric.

Fluids require more caloric to raise their temperature to the same degree than their corresponding solids. If this were not the case, if the solid required more heat for this purpose than the corresponding fluid, we could not render that body solid by diminishing the temperature, but might render it so by raising it. These bodies, if there be any such, would in their own nature be fluid, in the greatest degree of cold. If a body be capable of more changes in capacity than one, it will have more melting or freezing points than one, and we shall have the body in a solid form with different properties, and with similar differences when it is fluid. Whether there are any such is uncertain. The serum of the blood indeed freezes at  $25^{\circ}$ , where its capacity becomes smaller. But it recovers its capacity and fluidity when heated again above  $25^{\circ}$ . It becomes solid again if the temperature is raised above  $156^{\circ}$ , and as this is not performed instantly but gradually, its capacity though again become greater, and latent heat go into it to make it solid. It does not, however, recover its capacity upon the heat being diminished below  $156^{\circ}$ , but retains its solid form in all temperatures below this. Its appearance in this state is very different from that of the solid produced from serum at  $25^{\circ}$ . If the heat be increased above  $156^{\circ}$ , it again becomes fluid, but with properties. Serum of blood, however, is

not a homogeneous substance, and these changes are probably connected with some chemical action, among its component parts.

Sulphur also, in its liquid form, exhibits some phenomena, in its relations with caloric, which may possibly be connected with repeated changes of capacity. It has been long known to become very liquid at its fusing point, but at a higher temperature to thicken remarkably. I propose, however, to allude to this circumstance in the third part of these Essays.

This method of investigating the natural zero has, as might be expected, attracted considerable attention from philosophers. Objections have been started to various parts of the argument, and some of the later inquirers have been completely successful, at least in convincing themselves of its fallacy. At the same time, it has not thus been uniformly attacked, and I might add a list of its adherents, which should include many of the most candid and most distinguished of the philosophical world. In the succeeding part of this essay, it is my intention to consider the nature, grounds, and importance of these objections, and to answer them, and remove or explain doubtful points, as far as I shall be able.

The objections offered to the doctrines in this and the foregoing essays have been of two classes: they either tend to shew its fallacy, from the result of experiments made on various substances, to determine the minimum of heat, and which have cer-

tainly given points of depression for the natural zero, as wide of each other, as the warmest opponent of the doctrines could desire; or they attack the principles upon which it depends with direct arguments of their falsehood, or supposed deductions leading *ad absurdum*. As far as I have been able to consider the subject, none of these arguments will resist a strict scrutiny.

I shall now proceed to recapitulate them in order, pointing out where they appear to me to be defective. The more the subject is investigated, the more probability is there of attaining the grand end of all philosophical inquiries—the discovery and promulgation of truth. If this method of finding the natural zero be really wrong, it would be better that it were completely decided to be so. But it would not be better to abandon it upon inconclusive grounds, nor would the true interest of science be promoted by such a measure.

One of the principal causes of the doubt of some philosophers with regard to Dr. Irvine's theory has arisen from the very considerable, indeed extravagant discordance of the experiments and determinations of the natural zero. It seems at first sight naturally and fairly enough stated, that this theory, which proposes a method of discovering at what point a body shall be deprived of all heat, should, upon reference to facts or experiments, be found to coincide with them. This, however, is totally impossible, for if it were to coincide with one ex-

periment, that would only lead it the further from the next. But in this inquiry, it is not to be forgotten that we have no authority for the accuracy of these experiments. We may relieve ourselves from the dilemma equally well, by denying the truth of the experiments as that of the theory. Let us proceed to inquire which of these it is most just to do.

To form the ground-work of such an examination I here subjoin a state of the various points of depression found by different authors for the natural zero. Among these I do not include that of Dr. Irvine, which was about — 900 in the double instance of ice and water, and sulphuric acid and water. I omit these because I know he was far from putting that implicit confidence in his experimental observations which so many recent authors have the happy faculty of obtaining.

#### TABLE OF THE NUMBERS

*Expressing the natural Zero according to various authors.*

Dr. Crawford	— 1500
Mr. Kirwan	— 1318
M. Lavoisier	— 3426
Ditto	— 7260
Ditto	— 2598
Ditto	— $8\frac{1882}{1713}$ below 32°

The last of these numbers is fractional, and expressed in degrees of Reaumur's thermometer. It

is besides a negative quantity. I do not know exactly how it was obtained, nor what objections there may be against the process by which it arose. But it is justly remarked by Seguin that it would prove the fallacy of the hypothesis from which it is derived if the determination of the capacities were rigorously exact. This exactness, however, there seems very little reason to admit, as it shall be our object presently to prove.

It is clear that the natural zero ought to be at the same point in all bodies; that is to say, it must require the like number of degrees to raise any body to a given temperature. But these degrees, though numerically equal, are very different with regard to the quantity of caloric which they mark. In order then that all bodies should appear to contain an equal number of degrees of heat at the same thermometrical point, it is absolutely essential that these degrees in each body be computed according to its own capacity. For example: if the natural zero be calculated in the case of ice, taking the capacities of ice and water as 9 and 10, and the latent heat as  $140^{\circ}$ , the number obtained will be 1260. But this number cannot possibly coincide with any other determination, because these degrees are measured by the capacity of water. But they ought to have been proportional to that of ice, and the natural zero properly stated would thus be  $1400^{\circ}$ . It is by no means evident that this precaution has been duly attended to in constructing the tables of the real



zeros from which so many inferences have been drawn.

If the experiments above stated could be supposed to be perfectly accurate, and the arithmetical calculations just, the question might be regarded as decided. But before admitting such conclusions, it may be proper to pause a little and examine the steps by which these inferences were attained. In doing this, I shall first proceed to examine the various zeros found by Messrs. Lavoisier and Laplace, as obtained by similar and questionable means, and as differing the farthest from each other as well as from former determinations. The experiments of these gentlemen were performed by help of the calorimeter, an instrument for measuring heat invented by themselves, and of which a full description is to be found in Mr. Lavoisier's Elements of Chemistry. The quantity of ice melted is supposed to furnish precise grounds for computing the quantity of heat abstracted from the substances under examination. Now the truth of the experiments in question depends entirely upon the accuracy of this instrument. It is not a common accuracy that is required in this instance at least, when the results are employed to investigate the truth of Dr. Irvine's method of finding the natural zero. It will be seen that if the errors of the calorimeter amount to as much as a very small fraction of the whole, the inferences are confessedly unjust. But even in a theoretical view, very serious objections may be made against this way of deter-

mining the capacities of bodies for heat. The inventors of the instrument themselves acknowledge, that in spite of all their efforts a current of air passed through it, unless the temperature of the room was constantly kept at  $32^{\circ}$  of Fahrenheit. A very few degrees of variation, indeed, even one, either of increase or diminution, are sufficient to affect in a most material way the accuracy of the result. It must appear to every person familiar with the phenomena of caloric, how extremely difficult it must always be to preserve this equability of temperature for any length of time. The instrument besides seems, from its construction, liable to entangle the water formed among the ice, and sometimes to discharge a just quantity, sometimes from casual obstructions too little, and at other times these obstructions being suddenly removed, too much. It does not appear possible, but that these accidents should frequently occur. Nay, Mr. Wedgwood has even found, most unexpectedly, that the new formed water was again frozen and choked up the passage for the discharge of more. From these circumstances it may be inferred, that this instrument, however promising at first view, is by no means entitled to the pompous praises of complete accuracy which have been so liberally bestowed upon it. I do not mean to deny altogether the merits of this invention, which is probably useful for a great deal, though not for every thing. Much less is it my intention to refuse, if I even could, to its ingenious contrivers

that praise which they have so well deserved of the sciences. It is enough for me to shew at present, that this instrument may be fairly supposed to give results not mathematically accurate, and which are probably even more considerably wide of the truth than it is necessary now to prove.

Having said so much upon the defects of the calorimeter, I proceed to state, that Messrs. Lavoisier and Laplace have themselves, with great candour, allowed that very inconsiderable errors, amounting even to no more than one-fortieth part of the value of the capacities, would be sufficient to make the now discordant results coincide. They add, with a modesty worthy of such men, and strongly contrasted with the confidence of many inferior authors, that they cannot answer that an error so small may not have escaped them in their experiments. M. Seguin, upon this subject, observes, that since very direct inferences cannot be drawn from these results, it becomes necessary to have recourse to other proofs : these shall be afterwards considered.

These experiments of Lavoisier and Laplace, which have afforded zeros so extravagantly wide of each other, have probably produced more impression than any other arguments upon this question ; yet it appears, that when duly investigated, they are very far from being decisive. Indeed, they are no more than what ought to have been expected in a subject of so much nicety and complexity as this. It would have been still more extraordinary, if they had co-

incided, granting the theory to be just; in the present state of instruments for measuring caloric, no reasonable man could possibly look for accuracy so great as certainly to exclude errors of one-fortieth part. Some of M. Lavoisier's materials, as the nitrous acid, seem liable to many objections in all situations from the volatility of the acid. It may be worth while in a much easier case, that of ice and water, to compare the results in the hands of different philosophers.

If in this instance, we call the latent heat of water with Professor Wilcke\*  $57^{\circ} 33$  of Reaumur, or in Fahrenheit's scale, nearly  $129^{\circ}$ ; and if with Dr. Irvine, we take the capacities of ice and water to be to each other as 8 to 10, the natural zero computed according to these grounds will be found to be  $516$ . These are the lowest terms stated by authors. Let us now take the highest, and suppose with Mr. Kirwan, the capacities of ice and water to be to each other, as 9 to 10, and the latent heat to be as stated in Mr. Nicholson's System of Chemistry  $146^{\circ}$ ,† the natural zero turns out to be  $1314^{\circ}$  below  $32^{\circ}$ . Now, I appeal to every candid mind, whether the discordance of these results would not be regarded as complete proof of the falsehood of the theory, surely as much so, as the results already detailed, if the experiments had been made upon different substances. But they are upon

\* *Annales de Chemie*, Vol. 3d, page 171.

† Page 15, Edition, 1796.

the same, and it became necessary for those who discredit the theory, to refer the error to the experiments. This affords a sufficient illustration of the great effect of apparently small errors in these computations. But it is impossible to explain thoroughly the great difficulty of all experiments upon heat, a substance or property so fugacious and so easily transferable to all bodies that it cannot be confined or measured with unobjectionable accuracy.

It has been said, that if we deny the truth of these experiments, what foundation do we leave for the theory which rests upon similar experiments? To this it may be answered, that we only deny the perfect accuracy of experiments, but by no means the approximation to truth within a certain degree. They may be sufficiently near the truth to shew that certain properties of bodies exist; but yet so far removed from complete precision, as to make it impossible for the computed zeros to coincide; in a word, in the one case we need not be within a fortieth of the true number, in the other case, it is essential to be so. Surely there is a wide and fundamental difference between, on the one hand, observing that bodies have various capacities for heat, that they absorb heat during fusion; and at the same time have their capacities enlarged; and on the other hand, endeavouring to express the latent heat and the ratio of the specific heats in accurate numbers. We may come very near the truth; we may see clearly



that the properties in question exist, and we may draw legitimate inferences from such a data. But surely it is not consistent with any maxims of sound philosophy to say, that we can know nothing at all, because we are not in possession of a perfectly accurate and precise knowledge. We perceive that any body melting absorbs latent heat; we see that its capacity is augmented, and we conclude that the absolute heat of the body is in a certain ratio to these properties. We further assert, that if these numbers could be accurately found, the whole heat would be found also. And whether we discover or do not discover these points, the deductions are equally true.

It requires but a slight inspection and comparison of the tables compiled by different authors, to convince any person, that we are far from being able to give accurate numbers for the specific heats of bodies. Let us take Lavoisier's acknowledgment, that errors equal to one-fortieth part of the numbers expressing the capacities would remove all objections, and let us proceed to consider whether such errors do not commonly occur. At least it can be demonstrated that authors differ from each other greatly more than to this amount, and all cannot be right. In the following table are used, partly the numbers expressing the capacities of those substances, which have been employed for the determination of the natural zero, partly those of the capacities of other bodies.



TABLE

*Exhibiting a comparative View of the Differences of a few of the Numbers expressing the Capacities of various Bodies by different Authors.*

Substance.	Capacity	Author	Difference of capacities.
Ice	0.8	Irvine Kirwan	$\frac{1}{4}$
	0.9		
Sulphuric acid	0.4290	Crawford Lavoisier	$\frac{1}{4}$ plus
	0.3315		
Iron	0.1269	Crawford Irvine	$\frac{1}{4}$ plus
	0.1430		
Mercury	0.031	Crawford Thomson	$\frac{1}{16}$
	0.310		

These examples, which might be easily extended to a much greater length, to a length which should include every substance of which the specific heat has been examined by more than one person, will suffice to shew with what justice an appeal is made to the accuracy of experiments. In fact, there is no body of which the capacity can be asserted by the boldest believer to be determined nearly within a fortieth part of the true number. Much more concurrence, and experiments made with very large quantities of the materials, are indispensably required before any just conclusions can be drawn from the results with regard to the natural zeros. It appears to me then, that these arguments may be regarded as sufficient to prove, that the truth of this method of investigating the absolute heat in bodies is not

affected by the apparent discordance of some experimental inquiries.

We may now proceed to consider the second class of objections, those which affect the principles of the theory. The late learned Dr. Robinson, in his ingenious Notes on Dr. Black's Lectures, has stated on this subject, that the theory evidently presupposes the constancy of specific heats in low temperatures, or if they are not constant, that we know the laws of variation, and that these assumptions are highly improbable. I must differ from that learned gentleman with regard to the improbability. But as to the necessity of the constancy of specific heats to the theory, I absolutely deny its existence, and have already endeavoured, in the essay on Capacities, to shew, that it is by no means necessary or essential to our knowledge of the absolute heat in bodies. The analogical reasoning from the phenomena of chemical attraction is, in this instance, far from being well founded. Dr. Robinson, in the same place, adds, that it follows from this supposed inconstancy of specific heats, that even if it be granted that the heat given out during the freezing of water is the difference between the absolute heat of the ice at  $32^{\circ}$  and that of the water at the same point, still we cannot ascertain the natural zero. There is here stated a doubt, whether the latent heat of water is the difference between the whole heat of the ice and that of the water at  $32^{\circ}$ . I am at a loss to see how this can be denied, Ice at  $32^{\circ}$  contains, it will be al-

lowed, some portion of heat; whatever that is, we know also experimentally, as clearly as experiment can shew, that  $140^{\circ}$ , or thereabouts, enter the ice during its conversion into water. Perhaps I may be mistaken in supposing any doubt on the subject, but if there is, I confess, I am unable to understand the grounds upon which it is founded. I cannot help remarking also, that the assertion that Dr. Irvine could not make his experiments on congelations for the discovery of the natural zero coincide tolerably, and that these differed enormously from the results of experiments on mixtures is not perfectly just, as far at least as it is meant to apply to the experiments performed by Dr. Irvine himself. The truth is, that Dr. Irvine, in both these methods, made the natural zero nearly  $-900$ . But he was far from putting that implicit confidence in the accuracy of his observations that many philosophers have since done, and did not consider his experiments as by any means conclusive upon these points.

M. Seguin, in the *Annales de Chimie*, vols. 3d and 5th, has some observations of much ability upon this subject. I have already taken notice of the account that he there gives of the experiments of Messrs. Lavoisier and Laplace. After remarking that these experiments are not decisive, he proceeds to endeavour to demonstrate the insufficiency of the theory of capacities to determine the point of privation of all heat. In doing this, he lays down three propositions;

I. Caloric in certain circumstances obeys the laws of affinity, and combines with the molecules of bodies.

II. Capacities are not permanent.

III. The absolute caloric is not proportional to the specific caloric.

At least M. Seguin contends that all these three propositions do not exist at once. But they are all necessary to the doctrine of capacities: hence he infers, that doctrine cannot be true.

M. Seguin has certainly treated his subject with much ingenuity, and has collated into one view the various objections to the mode of accounting for the production of heat and cold, attributed by him and many of the continental philosophers to Dr. Crawford, but of which Dr. Irvine was the real author. The first of his propositions regards the entrance of caloric into the molecules of bodies by the laws of affinity. This question is, however, far from being so easily settled as M. Seguin supposes. Whether caloric does or does not obey these laws, must be considered as very doubtful. It is extremely uncertain if it really be matter at all. If it is not, it cannot be supposed to be under the influence of attraction. Even if caloric be material, it does not follow that it should. It is at best but a plausible conjecture, and depends altogether on another conjecture in itself in need of proof. It is impossible to admit with Seguin, that caloric united with his molecules does not affect the temperature of bodies,

and that consequently the real zero would not mark a total privation of heat. The quantity of caloric contained in a body, is known by its specific heat as well as by its temperature. If the change of capacity be the term at which heat is supposed to enter into the molecules, as in the case when ice at  $32^{\circ}$  is converted into water at  $32^{\circ}$ , we affirm that the temperature compared with the capacity, shews the absolute quantity of heat in the water as much as in any other instance. For the temperature of no body affords grounds to judge of its absolute caloric, unless account at the same time be also taken of its specific heat. When the capacity of a body does not change, either no heat enters into the molecules, or it goes in and comes out again according to a rule or ratio which we call the specific heat of that substance. Caloric never enters into any body without either raising its temperature or increasing its specific heat. To this general fact, too much attention cannot be paid. There is no proof, and no instance can be given, where caloric combines with the particles of matter without doing one or other of these. Therefore, though caloric should really combine with the molecules during fusion, or at any other time, provided only, that it combines in a manner of which it gives notice by the raised temperature or increased specific heat of the body, the determination of the real zero will not be affected by such a combination. And, if by patient and skilful efforts, all the experimental determinations of this point



should be brought to agree, we must not conclude, that caloric does not unite with the particles of bodies, but only that it does not unite out of proportion to their specific heats. It is perhaps, impossible to demonstrate the first of Seguin's propositions to be ill-founded, but the reverse of it can be rendered highly probable, and it is in itself without positive proof. It is difficult to see any absolute inconsistency between the doctrine of capacities and that of the chemical combination of heat. It is perhaps of little consequence, whether we know the law by which bodies seize, or that by which they only receive caloric, provided we only know the ratio. This part of our subject is extremely hypothetical and unsatisfactory, and may be abandoned with advantage. We cannot expect any precise answer to almost any question concerning the nature or mutual action of the ultimate divisions of matter. The doctrine of particles is yet involved in a dark, and it is to be feared, an impenetrable cloud. Whether the rays of science are one day destined to pierce this obscurity it would be fruitless to guess and presumptuous to determine. Yet, if the mutual action of three particles upon each other has puzzled and foiled the profoundest philosophers of Europe, how shall we hope to determine that question, which embraces the consideration of these minute bodies, infinite in their number, endless in the variety of their magnitudes and their forms, and agitated by a thousand motions of which we know neither the cause nor the degree?



If then a body absorbs heat in a regular way which we can investigate and foretell, as has been done in many cases, it is less necessary to inquire how this is performed. It is without all doubt necessary that caloric should enter into bodies regularly, or in a way which we can predict. But if it enters in such a manner, it is perhaps of inferior importance whether it does or does not combine chemically with the body. There are the strongest grounds for imagining that whenever a body changes its capacity it takes in or gives out caloric in such a way that the absolute heat still remains proportional to the capacity. At least among the almost infinite variety of chemical phenomena, where heat or cold is produced, in every case the specific heat seems to be accommodated to the alteration of the temperature, to become great when the temperature is diminished, and less when it is increased. If on case could be shewn where this does not happen, the argument must be abandoned. But till that is fairly done, it may be allowed to say, that there is here a most marvellous coincidence, if it really has no connexion with the phenomena.

There may be conceived to be three cases of the union of caloric with substances exposed to its action. Caloric may enter either by chemical combination, or by something different from this, which M. Seguin has called the admission of heat between the particles of matter, or it may enter partly in both

ways. But our argument will remain unaffected whichever of these modes of union is really consistent with facts, provided the heat can be discovered to enter according to any constant rule.

M. Seguin's second proposition, that the capacities of bodies are not permanent, I have already discussed, and shall not again enter upon here. I have endeavoured to shew, that the capacities probably are permanent, and that we have no just grounds to doubt this fact; but if they even are not so, that the theory is no wise dependant on such a supposition.

In like manner, the third proposition, that the absolute calories are not proportional to the specific, I have already fully discussed.

M. Seguin, after acknowledging that the experiments of Messrs. Lavoisier and Laplace, the results of which I have already stated, are not sufficient to determine finally the question of finding the natural zero, proceeds to draw deductions of a similar kind, but, as he thinks, infinitely more strong, from some experiments upon the combustion of phosphorus, hydrogen, and carbon. The details of these are to be found in Lavoisier's Elements of Chemistry. It was found that each pound of oxygen during the combustion of phosphorus in it, disengaged enough of caloric to melt 66.66 pounds of ice. Whence, supposing the oxygen to have parted with all its heat, and the water formed to contain  $135^{\circ}$  of latent heat, there must have been in the oxygen 8999' of caloric

in degrees measured by the capacity of the water. Or taking Dr. Crawford's determination of the capacity of oxygen at 4.769, that number applied to oxygen is reduced to 1887 nearly, which is the real zero according to the calculation. There are several circumstances to be taken into account before admitting the truth of this inference. It is to be recollected, that the experiment was made by burning the phosphorus in the calorimeter. All the objections to the accuracy of that instrument bear with full force upon this question. But even if that point were conceded, one yet more material remains. The whole of this reasoning hangs upon the justness of Dr. Crawford's determination of the specific heat of oxygen. If this be not fairly found, no argument can be drawn against the general theory or the application of it to discover the real zero. A very moderate alteration of the number expressing the capacity of oxygen would render this zero nearly the same with that of Dr. Crawford's own determination, from which indeed even in its present magnitude it does not very widely diverge.

Another determination of the natural zero is made by M. Seguin from the result of the combustion oxygen and hydrogen. It depends on similar principles to those already laid down. The first assumption is the accurate determination of the quantity of heat in vital air, to which may be added that of the precision of the calorimeter as a measure of heat. Such an accident as the temporary sacra-

ing, or any impediment in the course of the water in the conducting tube would readily account for a much greater quantity of caloric than what is here considered as existing in the discharged water. After all, the zero thus determined amounts only to 1612.72.

The experiments upon the combustion of carbon in oxygen are yet more hypothetical. For in that case, besides the former considerations, the capacity of carbonic acid is supposed to be ascertained. Upon the whole, it may be observed, that these objections apply rather to Dr. Crawford's statement of the capacities of different gases, and other bodies applied to the explanation of the source of animal heat, than to the general doctrines delivered in these Essays.

Of all substances none seem less fitted to afford an accurate knowledge of their relations with heat than the aëriform bodies. The difficulty of conducting experiments upon them is very great, while the sources of inaccuracy are unusually numerous. The purity of these substances is far from being easily ascertained, and they may readily give origin to erroneous ideas concerning heat from their contamination with various other airs, with solid substances which they can retain in solution, and from the very moisture which in an unavoidable but uncertain quantity never fails to accompany all gaseous bodies. One cannot help remarking here the extreme and extraordinary accuracy of M. Lavoisier's results. He never seems to suffer any loss in his experiments.



or at least only some evanescent fraction of a grain. That philosopher's high merits in every respect would alone protect him from any reflection of inaccuracy. Yet the mind is involuntarily struck with a combustion of hydrogen and oxygen, where six pounds ten ounces five gros and twenty-four grains of the gases are used, and six pounds ten ounces five gros and twenty-four grains of water produced. If this experiment would bear repetition with the same accuracy, it would afford the best possible argument against the gravity of caloric.

The only other objections to this method of discovering the lowest degree of heat which occur to me as worthy of consideration are those urged by Dr. Thomson, in his System of Chemistry. I have already noticed his observations upon the theory of capacities in general, and closely as every part of that theory is connected, it cannot be necessary to enter again upon this subject at great length. That gentleman, in the last edition of his work, vol. 1. page 398, has inserted the following passage.

"If the specific caloric of bodies has any meaning, it can only be, that the same quantity of caloric raises the temperature of one body a greater number of degrees than it does another. When we say that the specific caloric of A is  $\equiv 6$ , and that of B  $\equiv 1$ , what do we mean, unless that the quantity of caloric which raises B 6° raises A only 1°, or that what raises B 60° or 600°, raises A only 10° or 100°? When we say that the specific caloric of water is 10°, and

that of ice 9, do we not mean, that the quantity of caloric which raises the ice  $10^{\circ}$ , or  $100^{\circ}$ , raises water only  $9^{\circ}$ , or  $90^{\circ}$ ? Yet during the change of ice into water  $140^{\circ}$  of caloric enter it without raising its temperature; *a quantity greater than what can be accounted for by the difference of specific caloric by 126 degrees.* When water is converted into steam,  $1000^{\circ}$  of caloric disappear; yet the specific caloric of steam is to that of water, according to Dr. Crawford's own experiments, only as 155 to 100; *so that no less than  $48\frac{1}{3}$  degrees of caloric disappear, which cannot be accounted for according to this theory.*"

If this reasoning were well founded, the theory would indeed require no further confutation. But it is not very difficult to shew the source of error in this argument. The 126 degrees here mentioned, were obtained by Dr. Thomson, as we learn from a former work of his, in the following manner. He says, let the  $140$  degrees of latent heat in water be supposed to be only degrees measured by the capacity of ice, instead of that of water, to which they are really proportional. When therefore these degrees enter ice at  $32^{\circ}$ , the temperature ought to be  $158^{\circ}$ : for  $10 : 9 :: 140 : 126$ , but it was only  $32^{\circ}$ . Therefore  $126^{\circ}$  of caloric have, as Dr. Thomson asserts, disappeared, and cannot be accounted for by the change of specific caloric.

Dr. Thomson thus gives it as a fair statement of Dr. Irvine's theory, that the  $140^{\circ}$  entering ice during its change into water, should be lessened in the



ratio of 9 to 10, and ought therefore to be only  $126^{\circ}$ ; or in other words, if the heat entering the ice were only sufficient to raise the temperature of the ice  $140^{\circ}$ , the temperature of the water ought to be  $158^{\circ}$ . On this supposition, however, the water would contain two portions of heat, its original quantity from the natural zero to  $32^{\circ}$ , expressed in degrees measured by the capacity of ice, and the superadded portion from  $32^{\circ}$  to  $158^{\circ}$  measured by that of water. The ice, according to this statement, would no doubt melt without absorbing any caloric, and the water would be merely heated as in every other case. But Dr. Irvine, and after him Dr. Crawford, and the writers of Elementary Treatises, and I believe I may say all our philosophers, except Dr. Thomson, have stated this doctrine of capacities to be, that every one of the degrees, expressing the whole heat contained in a body is to be taken in proportion to the capacity of that body. And therefore that if ice suddenly changed its capacity it would absorb not merely a rateable proportion of what heat might be presented to it, but an absolute quantity to make up for its new capacity. For the quantity of heat necessary to raise the temperature of ice each degree from the natural zero, is to the quantity of heat necessary to raise the temperature of water each degree from the same point, as the capacity of the one body is to that of the other. Ice cannot then, as it acquires its new form, shew any increase of temperature, till

the differences between the heats of each degree from the lowest point be made. These differences amount to the  $140^{\circ}$  found by experiment, and cannot raise the temperature of the water even by the fraction of a degree, because it is barely sufficient for the demands of its new capacity. Had the  $140^{\circ}$  been applied to water, Dr. Thomson's assertion of the use of temperatures would have been just, but it can by no means be admitted, when alluding to a portion of caloric entering a body during a change of capacity.

It is obvious, that if we could instantaneously increase the capacity of any body, it would immediately become colder; and its temperature would sink proportionally as much lower as its new capacity was higher. In the same manner if a body has its capacity suddenly increased, and at the same time a quantity of heat added to it to make the absolute caloric proportional to its new capacity, that body would continue exactly at the same thermometrical point. But melting ice in such a body, the  $140^{\circ}$  are demanded by its new capacity, they would have been more, if its capacity had been greater, and less if less. On the very principles of the theory, therefore, ice, on becoming water, ought to receive a quantity of heat, and that quantity is not governed by the proportion of  $9 : 10$ , but the whole heats are in that proportion, and the  $140$  degrees are only their difference. All calculations on the alteration of tem-

perature to be produced by a given portion of caloric, a body from the knowledge of the relation of its capacity to that of another body, and of the number of degrees that other is raised by the same quantity, continue just only while the capacities continue in the same proportion. Dr. Thomson's observations upon steam are equally objectionable, and are to be answered precisely on the same principles. It would be needless waste of time therefore to extend further the consideration of this subject.

Immediately connected with the theory of heat which it has been my object to explain and illustrate in these Essays, is the account given of the origin of animal heat and of combustion, by Dr. Crawford. It is indeed so obviously dependant on, and so immediately derived from the doctrine of capacities, and particularly from that part explained in the following essay, that I am a little surprised to observe it asserted by several authors, some of whom are very recent, that Dr. Crawford's theory is derived from Dr. Black's speculations, when surely it evidently comes directly from the doctrines now treated, and only through them from the theory of latent heat. It wholly hangs on the previous admissions, that bodies contain heat according to their capacities, and that a change of capacity is always accompanied by a proportional change of temperature, propositions first, as I believe, enunciated by Dr. Irvine, certainly

method of investigating the natural zero proposed by Dr. Irvine, is derived from principles which have their foundation in nature, which can be fairly supported by argument, and which are not contradicted by the results of any experiments hitherto instituted.

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## APPENDIX TO ESSAY IV.

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Among the manuscripts of my father there were found the following definitions and propositions. They are obviously incomplete, and were probably intended to have been enlarged, so as to contain the whole of his doctrines. Notwithstanding this imperfection I have determined to insert the portion finished, since it appears to me to comprehend a condensed and accurate account of a considerable part of the subject of caloric as viewed by Dr. Irvine.

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THAT philosophers may reason with distinctness and perspicuity about heat, it is necessary that they should agree in the terms which they use to express the various measures which they apply to it.

The measures of heat known to us are, I think, reducible to three, to wit, temperature, relative quantity, and absolute quantity.

I. By temperature of heat in a body is meant that

which is indicated by the thermometer, this is always measured by the degree which the thermometer is raised by that temperature.

Temperature therefore is always expressed by degrees of the thermometer, and we may say the temperature of one body is equal, is higher or lower than that of another; and if higher or lower we may express the difference by degrees of a certain thermometer, but, until the ratio between one temperature and another be ascertained by experiment and induction, we ought to consider temperature as a measure which admits of degrees, but not of ratios. Nor ought we to use the expression of a double or triple temperature, these being terms which can convey no distinct meaning until the ratio of different temperatures be determined.

II. As it is now known that two bodies of different kinds, even when equal in temperature and magnitude, or equal in temperature and weight, may yet have different quantities of heat, it is necessary to have some term by which this difference of bodies with relation to heat may be expressed and measured; and this is what I call relative heat, or relative quantity of heat. The relative heat of a body therefore is always measured by the ratio which the whole heat contained in a body bears to the whole heat of some other body of the same temperature and magnitude, or of the same temperature and weight.



Thus if one measure of water contains as much heat as two measures of quicksilver of the same temperature, this is expressed by saying that the relative heat of water is double to that of the same bulk of mercury, or if one measure of mercury is equal in weight to fourteen of water, we may say that the relative heat of water is to that of the same weight of mercury as 28 to 1. Again, if seven pounds of water have as much heat contained in it as eight pounds of ice of the same temperature, this is expressed by saying that the relative heat of water is to that of ice as 8 to 7.

III. The absolute heat of a body is the whole quantity of heat contained in it. This is doubled when the body is doubled, tripled when the body is tripled, while the temperature and relative heat are unchanged.

*Corollary.* From this and the second definition, it follows, that the absolute heat of a body A to that of another B of the same temperature, is in the compound ratio of their magnitudes, and their relative heat in relation to magnitude; or in the compound ratio of their weights and their relative heat in regard to weight; and their absolute heats will be equal, when the magnitudes or weights are reciprocally as their relative heats in relation to magnitude or weight respectively. When I speak of the quantity of heat without any qualification, I always mean the absolute quantity.

## PROPOSITION I.

The quantity of heat which raises a body one degree in temperature is half of that which raises the same body two degrees, one third of that which raises it three degrees, and so in proportion either accurately or very nearly. This proportion is proved by the experiment of mixing equal quantities of water or any other fluid of different temperatures: the mixture is found to have always that temperature, which is an arithmetical mean between the temperature of the bodies before mixture.

The reason why this proportion is expressed as either accurately or very nearly, is that it may be doubted whether the liquor in the thermometer has not by equal quantities of heat expansions which bear the same ratio to the whole liquor, or whether it has precisely equal expansions.

*Corollary.* Different bodies are raised from one degree of temperature to another by a quantity of heat which is as the degrees of temperature added, the bulk of the body, and its relative heat taken conjunctly. For when the relative heat and bulk are the same, the quantity of heat given will be as the number of degrees of temperature added. When the temperature and relative heat are the same, the quantity of heat added will be as the

magnitude of the body ; and when the temperature and magnitude are the same, the quantity of heat is as the relative heat. Therefore, when none of the three is the same, any quantity of heat added will be the compound ratio of the three ; to wit, the degrees of temperature by which the body is raised, the magnitude of the body, and its relative heat in relation to magnitude.

## PROPOSITION II.

Having given the magnitudes, the relative heat in regard to magnitude, and the temperature of any number of bodies which mix without any change of their relative heats, to find the temperature of the whole mixture :

*Solution.* Let  $a, b, c, d$ , be the given bodies, and let the same symbols express their magnitudes : let  $d$  have the lowest temperature, and let the degree of temperature of  $a, b, c$ , above  $d$ , be  $l, m, n$ , respectively : Let the relative heat of  $d$  be expressed by unity, and the relative heat of  $a, b, c$ , compared with that of  $d$  in equal magnitudes be  $x, y, z$ , respectively.

Multiply the several magnitudes  $a, b, c$ , first into their respective relative heats  $x, y, z$ , and then severally into the degrees of temperature by which each exceeds the temperature of  $d$ , to wit, into  $l$ ,

$m$ ,  $n$ , respectively. Add the three products together, and divide the sum by the sum of all the four magnitudes, and the quotient will give the degrees of temperature by which the mixture exceeds the temperature of  $d$ , which in the present example will be

$$\frac{ax + bmy + cnz}{a + b + c + d}$$

This solution follows necessarily from the corollary of the first proposition.

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To complete this mathematical statement of Dr. Irvine's view of the theory of heat, several other propositions are obviously wanted. Of these, however, I have not been able to find any notes of the author's writing. That he did extend his speculations in this form, and that he was partial to this mode of considering the subject, I have good reason to believe. He was accustomed to make his chemical lectures the vehicle of his opinions, and trusting to the clearness of his ideas and the accuracy of his recollection, he committed to paper comparatively few of his doctrines. I hope, however, that this portion which I have been able to select will not be wholly unacceptable, though incomplete. It was at one time my intention to have endeavoured to extend these propositions to

what may be called their original length. From this however I have been dissuaded by the consideration that a full exposition of almost all the additional points is contained in the foregoing and following pages, though wanting that mathematical brevity and precision, which another form might have bestowed upon them.

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# **PART THE SECOND.**

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## **ESSAYS**

### **ON VARIOUS SUBJECTS.**

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**BY THE LATE**

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**“ Labor indefessus, oculi intentissimi, et manus nigrorent carbo-  
gum non aversantes, precipue hi, magistri mihi fuerunt.”**

Upon some occasions, substances, upon being mixed, become colder than they were before. By this means more heat can be taken out of bodies than by any other hitherto discovered. By diminishing heat in this manner, its extensive influence has been investigated. It is by such means that it has been experimentally proved, that quicksilver is really possessed of all the properties that are peculiar to metallic substances, and that it differs from all those bodies hitherto known, in requiring only a very small degree of heat to melt it. By mixing bodies in the greatest degrees of cold that appear in nature a greater is produced, and there is reason to expect, if the experiments by which mercury has been frozen were to be repeated in the intense colds of Siberia, that more remarkable effects would be produced, and that ether, spirit of wine, and air, the only bodies that have never been seen in a solid form, might be there deprived of their fluidity, and the opinion that the fluidity of bodies depends on heat, receive the most complete experimental proof.

The degree, to which this diminution of heat can be carried, is limited, if the conclusions drawn from my experiments for determining the natural zero be well founded. Probably long before they are cooled to this point, all bodies would become solid. It would be singular, if the density in all were the same, when cooled to this degree.

Though the heat produced by mixing bodies has not been so useful in enlarging our ideas concerning

heat, as the diminution of it by mixture has been, it has, however, been much attended to by philosophers on account of several curious circumstances in its production. Heat or cold is produced by the same substances according to the state they are in before they are mixed. In general it is by mixing two solids, or a solid and a fluid, that cold is produced. It is generally by mixing two fluids that heat is produced. Those solids, or the solid and fluid which when mixed become colder, if they are rendered fluid before mixture, will produce heat upon being mixed. Thus nitrous acid mixed with snow or ice becomes instantly fluid, and a considerable degree of cold is produced. If the snow or ice is melted before it is mixed with the aqua fortis, a considerable degree of heat is the consequence of the mixture.

The cause of these differences has excited the attention of the greatest philosophers. The singularity of the phenomena rather than any distant prospects of utility, attracted their notice. They have differed very much from one another in the cause which they have assigned for these appearances. They do not differ only among themselves; the explication given by any one of them is seldom consistent throughout. From some experiments made several years ago, a way presented itself of accounting for the heat produced in mixtures, different from any that had been thought of before, and at the same time, more agreeable to the phenomena. Many

others had before occurred to me, but on a nearer view were rejected, because they did not agree with the phenomena, and were not sufficiently supported by experiment. So many had successively presented themselves, and had then been rejected, that after a long and fruitless search, I at last despaired of success. I was about to leave inquiring after a subject which had hitherto been so barren, when accident suggested to me an explication, which, so far as I have examined hitherto, has the clearest experimental proof.

After all it may be said that the subject was not worth the trouble, and that any discovery, if, after all, any discovery be made, which the subject could afford, was too dearly purchased; that it is at most but speculatively curious and has little chance of being ever of any use to mankind.

These observations, which every one will naturally make, are no doubt true. But no person can tell to what purpose any experiments or discoveries may be applied. Many things which at first sight had as much the appearance of idle curiosity, have furnished some of the most useful inventions. The person who amused himself with the glasses of a pair of spectacles, and observed that, in a certain position, they apparently turned objects upside down, might be held as very idly employed, though the discovery of that useful instrument the telescope was the consequence of it. Nor would one have imagined, that he who employed himself in rubbing a piece of am-

ber, and who was delighted with seeing the straws dance around it, an occupation more like madness than philosophy; was making an experiment, that one day would lead to the explanation of some of the most remarkable phenomena in nature.

In giving an account of this subject, I shall differ from philosophers of the greatest name, and for whose characters I have the highest regard. This will not, I trust, be imputed to foolish vanity, but to that earnest wish for the discovery of truth, which ought to take place of every other consideration.

Fluid bodies, upon being mixed, are almost the only ones that become hotter. In a very few cases a solid and a fluid produce a considerable degree of heat. The degree of heat produced by mixture is different in different bodies. In some it is so violent as to make the bodies burst out into actual flame, though formerly as cold as the air in the coldest weather. On other occasions bodies, when mixed, apparently do not become hotter. Even in these, however, heat is produced in a certain degree; but in order to perceive it we must use an instrument for that purpose. In those cases where the heat produced by mixture is so intense as to set the whole on fire, the bodies have so completely united, and such other changes have taken place, that they cannot be again separated.

Some bodies which, on being mixed produce a very considerable degree of heat, retain, however, after their mixture so many of their original pro-

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perties, and are so superficially united, that they can be separated again so as to recover all their former qualities; so that on mixing them a second time, they will produce the same degree of heat as at first. The most remarkable of this kind are the mineral acids when mixed with water. In all of these mixtures a considerable degree of heat is produced. None of the mineral acids, on being mixed with water, produce so great an increase of temperature as the vitriolic acid. When both substances before mixture are as cold as ice at the freezing point, on being mixed, the heat is greater than that of boiling water. The degree of heat in this mixture varies according to circumstances. It is greater, when the acid is strong than when weak. It is greatest with the same acid, when the quantity of acid nearly equals that of the water.

The qualities of the compound of acid and water are a mean between those of the acid and water before mixture. None of the principles of which either body is compounded has been lost during the mixture, or has been changed into heat or light. The water and the acid can be again separated from each other, and in this separate state will, upon being mixed a second time, produce the same heat as at first.

The heat produced in this case cannot be accounted for, from any new arrangement of the principles which compose those bodies, but must depend on some other cause.



It is the heat produced in mixtures of this kind which I principally intend to treat of at present.

The heat produced in those cases where the bodies are decomposed, is more difficult to account for, and is more nearly allied to the manner in which heat is produced by the inflammation of combustible bodies. The production of heat is, however, probably, even in this case, partly owing to the same cause, which produces heat in the mixture of acid and water; that part of it I mean which is first produced, and which by disposing the ingredients of the bodies to separate, paves the way for the production of it in another manner. This, however, is but hypothesis, and as such, I wish to distinguish it. It is seemingly not easily capable of experimental proof, as we cannot get the bodies consisting of the same ingredients; after the heat has been produced as at first, and by comparing them together, discover whether in this respect they agree with the mixture of acid and water.

The heat produced upon mixing acid and water has been explained by authors in two ways, the one accounting for it from the violence with which the particles of bodies rush together, and the great friction which must thus necessarily arise. The other explanation of it is, that the bodies most disposed to produce heat in mixture, are such as are of a fiery and acrid nature, qualities which have been accounted for by supposing that a certain quantity of fire was contained in those bodies, which meeting

with fluids like water, immediately produced the commotion, and the noise and smoke which water when thrown upon a fire produces. The first opinion is generally received by natural philosophers, and has been adopted by those of the greatest name. The second opinion has been proposed by some of the chemists, and received by a few of that profession in France. A little attention, however, will shew that neither are sufficiently supported by experiments.

At the head of those philosophers who have accounted for the heat produced by mixing certain bodies from friction, is Sir Isaac Newton, and he seems to have admitted it without sufficient examination as a consequence of attraction: and there is this further to be urged in excuse, that it is thrown out in the form of a query, as if he did not intend that much attention should be paid it. "When two bodies unite together, does not this (says Sir Isaac,) arise from an attraction which takes place between the small particles of those bodies? and when water and vitriolic acid poured successively into the same vessel grow very hot in the mixing, does not this heat argue a great motion in the parts of the liquor; and does not this motion argue, that the parts of the two liquors in mixing coalesce with violence, and by consequence rush towards each other with an accelerated motion?" In some other part of his queries, he seems to think that the vibratory motion is that in which heat consists. The opinion, that heat consists in the vibratory motion of the heated body, and that every such

motion was attended with heat, had been proposed by philosophers before the time of Sir Isaac Newton. Bacon and Boyle seem both to have adopted this opinion. In proof of it, they have adduced many of the commonly known methods of producing heat. Most of the facts brought to support it, are instances of heat being produced, when the particles of solid bodies are set in motion, such as the hammering of iron, till it becomes red hot, methods of kindling fires among savages, by rubbing two sticks together, and the firing of coach and mill wheels, that are driven with violence without being oiled. As heat and motion, by these facts, appear nearly connected, heat was concluded to be motion, and the heat produced in mixing fluids was from analogy concluded to arise from the same cause. But it is one of the many instances that might be given, how little trust ought to be paid to analogical reasonings in physical subjects.

That heat is not produced from the motion of fluids is clearly supported by experiment. Let us throw any fluid we please into the most violent commotion, no heat is produced, that can be discovered even by the most sensible thermometers. At least, this is the case if the fluid is in the same situation and possesses the same properties after, that it did before the motion. If it is changed by the motion, if a part of it that was fluid has become solid, then heat will be produced. But this is never the case with simple bodies, such as water, mercury, and

the like. It is only in very compound fluids that motion produces heat, such as milk, and the solution of certain salts in water. Milk, when agitated in the common process of churning, becomes hotter, but in proportion as it heats, a quantity of butter separates from it, and instead of being completely dissolved and fluid, assumes a solid form, and the whole after the agitation is very different from what it was before. When a quantity of Glauber's salt is dissolved in warm water, and the whole is allowed to cool without being disturbed, upon shaking the solution it grows hotter, but a part that was fluid is instantly made solid. So little are the simpler fluids disposed to produce heat by being set in violent motion, that they hinder solid bodies when their particles are moved from producing heat. A coach wheel which by being moved with great velocity round its axis without oil, will produce heat sufficient to set the whole on fire, upon being oiled will produce little or no heat. If the effect of the oil be in this case to hinder any violent motion being excited in the particles of the solid bodies, it is equally evident, that from the great ease and readiness with which the particles of fluids slide naturally over each other, that they are scarce capable of being thrown into any tremor, as the particles of solid bodies from their nature may easily be. It is not very certain whether, when the particles of solid bodies are set in violent vibratory motion, a quantity of heat can with much propriety be said to be produced; as this seems to

imply its being brought at that time into existence. Perhaps it would be more proper to say, when the particles of solid bodies are put in a violent vibratory motion, that a quantity of heat is collected from the surrounding bodies; in the same manner as a glass globe on being rubbed, and having a similar vibratory motion excited, collects from surrounding bodies a quantity of that electrical fluid, which they all in a natural state contain. Were we to indulge conjecture, we might say, that by exciting a vibratory motion in the particles of solid bodies we produce a momentary change in their nature; in consequence of which, that substance, which is the cause of heat and which they all contain, is capable while the motion lasts of producing a greater effect than it could do when the particles of the body were in a quiescent state. But were this the case, the moment the motion ceases, the body should be as cold as it was at first. This is not so; the body preserves its heat as long as on ordinary occasions. May not this therefore be accounted for by supposing that friction does not leave the body after it ceases in the same state as at first?

If the heat produced on mixing fluids depended on the friction which takes place in consequence of the attraction which subsists between the particles, and which disposes them to run towards each other with an accelerated motion, we should expect that no change would be produced in the body, unless the friction was so violent as to alter the figure of

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the particles of the body, in consequence of which, the qualities of the compound should not be a mean between those of the ingredients. This is not supported by experiment; for the mixture of acid of vitriol and water seems to possess qualities exactly a mean between the acid and water before mixture, except where heat is concerned. Examined as ~~affected by heat~~, the qualities are so different, that it could not arise from any accidental jostling of the particles, but must be owing to some permanent change in their properties. Besides a compound exactly similar in every respect to an acid and water, may be produced by an acid, and snow or ice; yet in this case, the union of the ingredients is not attended with heat; on the contrary, they become colder than before mixture. To account for this, we must suppose either that the attraction which disposes them to unite is changed to repulsion, or to something opposite at least to attraction. But what shall we substitute in place of the vibratory motion which produced heat when acid and water are mixed? There is nothing opposite to motion but rest. So that this doctrine supposes that rest which in one case is neither disposed to produce heat nor cold, in another, becomes active in producing cold. At the same time, we see no appearance of repulsion between the particles of the ice and acid when cold is produced; on the contrary, so far as we can judge from appearances, the union is completed as soon when cold as when heat is produced.



The second account is, that those bodies which in mixture produce heat, contain a quantity of fire in them, and to this is owing the hissing noise when they are mixed with water, and the heat which they produce with it. Lemery, in his *Chemistry*, was the first who proposed this doctrine, and it was generally received by the French chemists. He said that the acid had carried over this fire with it in distillation, that the acid particles were confined in it till set loose by the water, when they separated with violence, and appeared in the ordinary form of fire, incapable of confinement. No proof is given of this doctrine; and when the author observes as an objection to his theory, that some may deny that there is any fire in the acid, the answer that he gives is, that it is very probable that there is fire in the acid; and besides without this fire it is impossible to account for the corrosive qualities of the acid as well as its power of producing heat with water. This doctrine, which Lemery published in 1687, M. Beaumé, in his *Elements de Chimie*, has lately proposed as new, and begs that its novelty may be no objection to its being received; and particularly insists that the opinion may be allowed him, though he cannot explain how this union of fire and acid is effected. He is not however without hopes that in time he will be able to determine the exact quantity of fire which vitriolic acid, as well as the rest contains. He expresses the situation of this fire in vitriolic acid by the term

"*feu legerement bridé*," and when set loose by the water added to it, he calls it "*feu pur et isole*." No experiment is brought in support of this doctrine. The existence of fire in this state is not proved. By so large a portion of fire escaping, we should expect great alterations in the qualities of the substances. Besides, water seems to be but ill calculated for breaking these bonds which connect fire and the acid; for water, to do this, should have a repulsion to fire: on the contrary, heat is attracted by water with great force. If this bridle were destroyed by the water, the greater its quantity the more heat should be produced, which is not confirmed by experiment. Besides, the same compound is produced by adding to an ounce of vitriolic acid eight ounces of ice, that is produced by adding eight ounces of water; yet the one produces cold, and the other heat. All the particles of fire which the acid contained when ice is used should be retained in the mixture, while a part is dissipated, when water is employed: this should occasion a difference in the qualities of the two compounds, which experiment does not support.

This however may be accounted for in the manner of these chemists, and equally philosophically, by saying that in ice there is a quantity of *froid bridé*, which destroys the *feu bridé* of the vitriolic acid. This is exactly similar to those authors who, to account for darkness, imagined there were certain

Stars which rayed out darkness in the same manner as others ray out light.

The reason why heat is produced by mixing fluids seems to be, that the ingredients which compose the mixed fluid are in a different state with regard to heat from what they were before. It would seem that whatever is the cause of heat, it has a greater effect in heating the bodies after the mixture than it ought to have had in proportion to its power of heating the ingredients before mixture. Two bodies, which being mixed have produced heat, are for ever after this much more easily heated and cooled, than we should have been led to expect from the properties of the ingredients before mixture. Thus a mixture of acid of vitriol and water when set before a large fire, becomes as hot as the fire can make it in a less time than in proportion to the ingredients. Vitriolic acid in a phial, and an equal bulk of water in a similar phial, being set before a fire, so that the fire shall act equally on them, the vitriolic acid will be heated as hot as the fire can make it in less than one half the time the water will be, though at last both will be equally hot. One of these phials filled with a mixture of equal parts of acid and water, and placed in the same circumstances, should require longer time to heat than the phial with acid, and less than that with water, and the time it takes should be as much more than the acid took, as it is less than the water took. This by experiment

is not the case. The time which it takes is much less than in this proportion, and its heating or cooling is nearer the property of the acid than that of the water. From this it appears that when heat is applied to the acid and water after mixture, it either produces a greater effect on this than it did on the same matter when the bodies were separate, or that the bodies when mixed attract heat with more force in the mixed state, so that by running with more velocity, less time is required for the necessary quantity to go in.

The first of these is the case as appears from the following experiments: mix vitriolic acid and water in equal parts; call this the mixture. Take three vessels of equal size and like shapes, and into the first put a quantity of vitriolic acid; into the second an equal quantity of water; and into the third an equal quantity of the mixture. Let the three vessels with their contents be heated to the same degree, so that all of them may be  $100^{\circ}$  hotter than a quantity of glass dry and in powder. Throw into each of these vessels a quantity of the cold glass, till each becomes fifty degrees colder than it was, while the glass added becomes fifty degrees warmer. The quantity of glass necessary for this purpose will be proportional to the quantities of the cause of heat necessary to heat each of these bodies fifty degrees, or the same quantity of the substance, which is the cause of heat, applied to the same quantity of glass will

produce the same effect, and a less quantity of heat will be necessary to heat a small quantity of glass than a great one. Hence the cause of heat has a greater effect in producing heat when applied to the matter of vitriolic acid, than when applied to water in the proportion of the quantities of glass. For three pounds of glass have as much power in cooling a quantity of acid of vitriol as five pounds have on an equal quantity of water. But the effects which the cause of heat produced on the two bodies are equal; therefore the quantity of that cause is as the quantity of glass heated by the fifty degrees lost by the water, to the quantity of glass heated by the fifty degrees which the vitriolic acid lost, or as 5 : 3. If the acid and water, when mixed, were disposed to be equally affected by the cause of heat as they were disposed before mixture, then a mixture of equal parts of acid and water heated an hundred degrees hotter than the glass, and taken in the same quantity, and in the same vessel, should require four pounds of glass to cool it fifty degrees. It will be found by experiment that three pounds and a half will be sufficient for this purpose; hence the mixture is more easily cooled fifty degrees than it should be in proportion to its ingredients, and that in the ratio of 4 to  $3\frac{1}{2}$ , and the heat which is lost had a less power of heating than it ought, because these fifty degrees instead of heating four pounds of glass, as they should have done,  $50^{\circ}$ , have only heated three and a half pounds to the same



point. Hence there is less of the cause of heat in the  $50^{\circ}$  which the mixture lost, than there would have been had the mixture preserved the qualities of the ingredients. And for the same reason that there is less of the cause of heat employed in the  $50^{\circ}$  than there ought to have been, there will be less of it employed in 500 or 5000 degrees. From this it seems reasonable to conclude, that whatever is the cause of heat, it is capable of producing a greater effect in heating the mixture than it ought in proportion to the ingredients. The acid and water before mixture contain such a quantity of the cause of heat as is sufficient to produce all the heat in them from the lowest degree of heat to the degree at which they are mixed. The moment that the mixture is made, the body is so changed, that the same quantity of the substance which is the cause of heat is capable of producing upon the same quantity of matter a greater effect than before, and this in the proportion of  $3\frac{1}{4}$  to four, or as 7 : 8. Whatever effect therefore it would produce before mixture, it will after mixture produce one-seventh more. Hence, if the two substances before they were mixed contained such a quantity of the cause of heat as is sufficient to produce when they are at  $32^{\circ}$  of Fahrenheit, an effect equal to seven times the difference between the heat of frost and that of boiling water, or seven times  $180^{\circ}$ , after mixture, it will produce eight times this difference, or eight times  $180^{\circ}$ . The two substances



therefore, though as cold as frost before mixture, will be as hot as boiling water after it.

If the proportions of acid and water be different from that of equality, the quantity of heat produced will be different. Thus one part of vitriolic acid mixed with eight parts of water becomes only  $26^{\circ}$  warmer, whereas from what happens when equal parts are mixed, a good deal more ought to have been produced. From which it should follow, that the cause of heat does not produce an effect so much more in proportion to the ingredients, when the proportions are as 1 : 8, as when the acid and water are mixed in equal quantities. And this seems to be proved by mixing this compound of acid and water in the proportion of 1 to 8 with more acid ; when we shall find that the whole, though as cold as the air before mixture, becomes exceedingly hot after it, but not so much so as if this quantity of acid had been mixed with an equal quantity of pure water. If the substance which is the cause of heat, instead of producing a greater effect in heating the mixture than it ought to have done, in proportion to its power of heating the ingredients before mixture, had not been able to heat it so much as it ought to have done, and this by an opposite change in the bodies ; then, upon being mixed, the whole would have become colder than the ingredients were before mixture, and cold instead of heat would have been produced. Thus if the cause of heat, instead of pro-

ducing a greater effect in the mixture than it ought to have done in proportion to its power of heating the ingredients, and this in the ratio of 7: 8, should produce a less effect in the proportion of 8: 7, then the cause of heat which was capable of heating the ingredients before mixture, from the lowest degree of heat to that at which they were mixed, would, after the mixture, have produced less by one-eighth part, and one-eighth part of the heat in the body would disappear upon mixture. If the two bodies, when mixed, were as hot as boiling water, and the whole effect of the cause of heat equal to eight times the interval between the freezing and the cooling points of water, the bodies, after mixture, should immediately become as cold as frost, or  $180^{\circ}$  of heat would disappear from the body.

I do not at present recollect any instance of this happening in nature, where two fluids, on being mixed, become colder, unless perhaps the solution of certain oils in spirits of wine be of that kind. But we have many instances of it, when solids are dissolved by fluids, as in the solution of salts in water, during which, except in one or two instances, the heat after mixture is less than before mixture. If the preceding account is well founded, this should be owing to the cause of heat having a less effect on the same quantity of matter after mixture than it had on the bodies before. Something of this kind frequently happens independently of mixture,

when the same body is in a solid or in a fluid state; as in ice and water, solid spermaceti, and melted spermaceti; lead in a solid and lead in a fluid state, and the like, in all of which, the substance which is the cause of heat has a greater effect in producing heat upon the solid than it has upon an equal quantity of the corresponding fluid. If the solid becomes instantly fluid, the substance which is the cause of heat produces a less effect, and the fluid becomes colder than the solid body before this change took place. And in the same manner, if the body is fluid and is instantly made solid, the substance which is the cause of heat is capable of producing a greater effect than it did before, and the solid becomes hotter than the fluid was before the change of form was effected. This is confirmed by experiments on ice and water. When ice is mixed with an acid, the ice melts, and a compound of the same proportions is produced, as the same acid and water would have done. But the acid and ice become colder after than they were before mixture; the acid and water become hotter. This may be accounted for in the following manner:

The substance which is the cause of heat has a greater effect in producing heat in the mixture of acid and water than it should have upon these bodies in a separate state, as has been frequently observed. But there is not so great a difference in this respect between the mixture, and the ingredients before

mixture, as there is between ice and water. So that the cause of heat, though in acid and water it produces a greater effect than it ought to do in proportion to the ingredients, in the mixture of ice and acid, may produce a less effect than it ought to do. The same quantity of the cause of heat contained in the bodies before mixing, will make the one mixture hotter than the ingredients were before, and the other colder. This gives the reason, why water at 32 mixed with nitrous acid at 32, produces heat, and why ice at 32 and nitrous acid at 32, produce cold.

If, when the acid and water are mixed in one proportion, the cause of heat produces a greater effect than the difference there is in this respect between ice and water; and when they are mixed in a different proportion, produces a smaller effect than the difference between ice and water; then ice and acid mixed in the first proportion will produce heat, and when mixed in the last they will produce cold. This seems to be the case with vitriolic acid and ice. Equal parts of ice at 32, and oil of vitriol at 32 become hotter, because the substance which is the cause of heat produces a greater effect on the mixture than it ought to have done in proportion to the ingredients. When they are mixed in the ratio of one part of acid to eight of ice, the mixture is colder, because the cause of heat produces a less effect than in proportion to the ingredients. The

change in the first case is greater than the difference betwixt ice and water, and in the last case it is less: Thus, if the effect which the same quantity of the cause of heat produces on the ingredients be to that which it produces in the mixture of equal parts of the two substances, as  $7 : 8$ ; while the effect which the same quantity of the cause of heat produces on water is to that which it produces on ice as  $9 : 10$ , then the difference of those ratios will express the effect which the same quantity of the cause of heat will produce on the mixture of ice and acid different from what it should have produced in proportion to the ingredients; and will be expressed by the ratio of  $\frac{7}{8}$  divided by  $\frac{9}{10}$  or  $\frac{70}{72}$ , that is, as  $35 : 36$  instead of  $7 : 8$ . So that less heat will be produced in this case than if water had been used. But still  $\frac{1}{37}$  of the whole heat in the bodies at the time they are mixed will be produced. Again, if the effect which the same quantity of the cause of heat produces in the mixture of one part of acid with eight of water be greater than what it ought to have been in proportion to the ingredients, as  $10 : 11$ ; while the effect in water and in ice is as  $9 : 10$ , the difference of those ratios will express the effect which the same quantity of the cause of heat will produce in the mixture of ice and acid from what it would have produced in the mixture of acid and water, and will be  $\frac{10}{11}$  divided by  $\frac{9}{10}$  or  $\frac{100}{99}$ . The same quantity of the cause of heat will



produce a less effect in the mixture of ice and acid in the proportion of 100 : 99, or one hundredth part of the heat contained in the ice and acid before mixture will disappear after it. Or it may be conceived thus ; the quantity of the cause of heat in water at 32 is to the quantity of the cause of heat in ice at 32 as 10 : 9. If the ice was instantly to become water, one-tenth part of the effect of this cause would disappear, or one-tenth of the heat in the ice would disappear when it became water. If to the water thus cooled an equal quantity of vitriolic acid be added, the effect of the cause of heat would be increased in the proportion of 7 : 8, or the whole would become hotter by one-seventh of the heat in it. Now as the heating cause one-seventh is greater than the cooling cause one-tenth a quantity of heat equal to this difference will be produced.

And that something of this kind happens, seems evident ; because when eight parts of ice at 32° are mixed with one part of vitriolic acid at 32°, the whole becomes immediately 44° colder. And if to this mixture a quantity of more acid be added, the whole becomes hotter by many degrees. The numbers which I have used are not exactly those given by experiment, because I wanted to avoid large numbers or fractions, which would have embarrassed a detail sufficiently perplexed naturally. Those I have employed are the nearest integers to the real numbers.



Some apology may be thought necessary for entering so minutely into the subject, and using many more illustrations than were essential. The only one I have to offer is that I found it impossible to explain myself clearly otherwise, and after all I may have failed in this respect. I would willingly run the risk of being sometimes thought prolix, were I sure of being perspicuous.

I have used the term cause of heat rather than latent heat, or any other term in deference to the opinion of those philosophers who conceiving heat to be quality, conceive latent heat to be a latent quality, and that a latent quality is no quality at all. I have used the term cause of heat, though I do not know what that cause is. It surely, however, has a cause, and whatever this may be, it cannot be subject to its own effect. All that we know is, that this cause produces different effects in heating different bodies. It produces a greater effect on mercury than on water in the proportion of two to one, when equal bulks are taken, and a greater effect on glass than water in the proportion of five to one, if there is any truth in the experiments which I have made. It appears that it produces different effects even on the same substance in different forms. It does not produce effects in proportion to the quantity of matter in bodies, since if this were the case it should produce when applied to equal bulks of mercury and water, only  $\frac{1}{10}$  part on the mercury which it

does on the water; whereas by experiment it produces on equal quantities of matter of the two bodies 28 times as much upon the mercury as it does on the water. In this respect it is very different from the substance which is the cause of gravity, which acts equally on equal quantities of matter. So that the gravity of the whole earth depends on the effect of this cause acting on every particle of it in proportion to its quantity of matter. In consequence of which the power by which one heavy body attracts another, diminishes as the squares of the distance increase. How the cause of heat which affects equal quantities of matter differently, should in a number of bodies equally heated have the power of heating bodies at a distance which diminishes in the same proportion, I do not know. I am equally ignorant whether the heat produced in those mixtures, where bodies are inflamed and consumed, and never capable of exhibiting the same appearances again, be owing to the cause which I have just assigned. It is very difficult to contrive experiments that will shew whether or not this is the case. There are even some experiments on the heat produced by mixing fluids that are not destroyed, which it is difficult to understand according to our theory. Whether these depend on some accidental circumstances, or some other property of heat, must be left till experiments have furnished a sufficient number of facts of this kind. Whether the sub-





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## ESSAY II.

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### *ON THE EFFECTS OF HEAT AND COLD ON ANIMAL BODIES.*

Read A. D. 1769.

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**T**HE bodies of all animals consist of solid and fluid parts. The solid parts contain a number of vessels ramified to an amazing minuteness through all the parts of the animal body. In these the fluid parts are contained, and are constantly circulating through them with great rapidity. The mechanism by means of which this is performed I had occasion to take notice of in a former essay, where I observed that though this was one of the most important parts of the animal economy, yet it was found by dissection, that it was very different in different animals. The motion of the fluid in these vessels depends ultimately upon the nerves, and according to their greater or less degree of sensibility this cir-

culatation will be fast or slow. But though in all animals the influence of the nerves on the heart and arteries is the principal cause of the motion of the fluids contained in these vessels, yet this may be greatly effected by other causes, as heat or putrefaction. Every alteration in the consistence of an animal fluid must affect its motion in the animal body. If the consistence is diminished, or the fluid rendered thinner, the same nervous influence will produce a quicker motion. If the fluid is by any means rendered of a thicker consistence, every thing else being unaltered, this motion will be slower. The alteration in the consistence of animal fluid depends chiefly on the degree of heat, though in some measure also on other circumstances. If the heat is greatly diminished, all the animal fluids will assume a solid form, in which case their motion must immediately cease. If the heat is considerably increased, they either become too thin, and consequently move with too much rapidity, or they undergo a change similar to that produced by cold. Any great alteration of heat, therefore, must produce great alterations in the motion of the fluids contained in the vascular parts of an animal body. The blood, which in most animals is of a red colour, is the most important of all the fluids, and is of the greatest consequence, as from it all the other fluids of an animal are separated, and how different soever they may appear in taste or smell, or other qualities, either existed already completely formed



in the blood, or were produced from it by a small alteration in the arrangement of its principles.

The blood of all animals that have been examined seems to divide itself into three principles. When it is allowed, after being separated from the body of the animal to cool, undisturbed in the open air, it resolves itself into a watery liquor, and a thick liver-like substance of a red colour, which upon being washed becomes white, the colouring matter being separated by the water. The blood cannot be again formed by uniting these substances together. The coagulum of the blood will not again dissolve in the watery part, though it is heated to the same degree as the body of the animal.

The watery part of the blood, or, as it is generally called, the serum, upon being exposed to a moderate degree of heat, considerably below that of boiling water, coagulates, or becomes solid, and never after can be made to assume a fluid form. If the serum is exposed to a frosty air, it becomes solid at  $25^{\circ}$  of Fahrenheit. The blood of all animals, however, does not freeze at the same degree; for though that of the more perfect animals freezes at  $25^{\circ}$ , the blood of some of the insect tribe remains fluid at  $4^{\circ}$ , or even lower than this. Therefore the blood of most animals can never suffer an alteration in heat equal to this, without being rendered totally unfit for circulating through the blood vessels. And consequently, no animal body can be heated thoroughly to  $156^{\circ}$ , and few cooled to  $25^{\circ}$ , without

having their circulation stopt, though nothing should in this case be affected but the fluidity of the blood.

In order to prevent the inconvenience, which would be frequently in danger of happening from too great a degree of heat or cold, nature has endowed many animals with a power of maintaining a certain equal degree of heat, nearly a medium between the two degrees, at which the blood would lose its fluidity. The animals that have not this power in any remarkable degree are placed in such circumstances, as to be in no danger of suffering either by too great a degree of heat, or too great a degree of cold.

Animals, therefore, are divided into hot and cold. Those animals being called hot whose heat is near our own temperature, while all the rest are reckoned cold, whose heat is much below ours, and which consequently affect us with a sensation of cold. It appears, however, from a vast number of experiments, that even those animals, which to us feel cold, have blood somewhat warmer than the medium in which they live.

Haller observes, that all the animals, whose blood is considerably warmer than the surrounding medium, have lungs like the human lungs, their hearts have two ventricles, they have a double circulation, and they require fresh air like inflammable bodies on fire. Of this kind are the quadrupeds and birds, and the fishes called cetaceous, as the whale and

porpus. The heat of these animals is from 96 to 108. Man is among the lowest of the hot animals with regard to heat. In a healthy state his heat is between 96 and 98. The common quadrupeds as sheep, dogs, cats, oxen, raise a thermometer applied to their skins four or five degrees higher, to 103° or a little more. The whale raises the thermometer about 101° or 102°; the heat of the porpus is 102, of the sea calf likewise 102. In these aquatic animals, as well as in the ordinary quadrupeds, the heat of the skin is generally a degree or two below that of the more internal parts.

As the structure of the lungs and viscera has a remarkable resemblance in these animals, so we find a resemblance in their heat. All the bird kind are very warm; they are among the warmest of the hot animals, and are generally hotter by three or four degrees than the animals just mentioned. Ducks, geese, hens, pigeons, partridges, swallows, &c. with the bulb of a thermometer in their groin, raise the mercury to 103°, 104°, 105°, 106°, 107°, and a hen hatching eggs to 108°. We do not find that the heat of any animal we are acquainted with ever exceeds this in a healthy state. The heat in the bodies of these animals is extremely uniform, seldom varying above a degree or two in all the various alterations of the air. When the bodies of animals are diseased indeed, this heat is liable to some change. A high fever will sometimes produce a heat equal to 108.

We can form no idea of the degree of heat in our own bodies by our feelings. A person shivering, and under all the sensation of the bitterest cold, in a fit of the ague, is frequently found by the thermometer to be two or three degrees warmer than in health. Dr. Boerhaave imagined that the heat of hot animals approached very nearly to that at which the serum of the blood coagulates, and the great danger which attends a high fever he supposes to be owing to the risk of the serum coagulating. But this, as I observed a little above, is far from being the case. For it requires a heat near  $156^{\circ}$  to coagulate the serum of the human blood, a temperature to which the body of no animal can endure to be raised. From this it is evident, that something necessary to life is destroyed by a less degree of heat than what is capable of coagulating the blood. This is the nervous energy.

We find that we are scalded by water at a heat still considerably below that at which the blood would coagulate. Animals differ more in this respect, than in the heat at which the serum becomes solid. A degree of heat which can be supported with ease for a considerable time by one animal will, in a few minutes, scald another to death. It is by means of this alteration in the nerves, that different animals bear different degrees of heat, and are so fitted for the different climates in this globe. Thus the birds, which inhabit our climate can, all of them uninjured, bear for a considerable time, a heat equal to  $100^{\circ}$ ; while

Some of the Greenland birds; upon being brought into the cabin of a ship, which may be about  $46^{\circ}$  or  $48^{\circ}$ , die in a few hours. The porpus again, whose blood is at  $102^{\circ}$  or  $103^{\circ}$ , can bear for some time water as hot as  $96^{\circ}$  without injury, while the same degree of heat would scald a perch to death in two minutes. Even the individuals of the same species differ in the degree of heat which they can bear without scalding. I have seen people put their hands with ease into water above  $150$ , without being hurt by it, while it is intolerable to me at  $114^{\circ}$ . It is probable from many experiments which I have made, that persons whose hands are not rendered callous by hard labour will find  $114^{\circ}$  scalding hot. The warmer animals of our climate can probably bear a greater degree of heat without pain than we can.

In general the nerves are incapable of being thoroughly heated, without injury, to any degree much above the heat of the blood. The heat can never probably in any disease be greater than  $114^{\circ}$ , and therefore an increase of heat never can produce death by coagulating the blood, but will, long before it arrives at the point necessary for this purpose, occasion death by affecting the nerves, and depriving them of their sensibility and office.

The animals which feel cold to us have still a power of generating a small degree of heat, though greatly below that of the hot animals, seldom exceeding four or five degrees above the medium in

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which they live. Of this kind are all the fishes, except the cetaceous, snails, frogs, and land and sea tortoises, toads, vipers, and all the serpent kind, and most insects. The frogs and tortoises have lungs as well as the hot animals, but they are of a very different texture, and the blood can easily circulate through their bodies, without passing through the lungs at all. The degree of heat produced in these animals is not the same in all. The less perfect the animal, the lower the degree of heat. Hence shell-fish, as oysters, muscles, &c. have scarcely any perceptible heat above the surrounding medium. Fishes of a more perfect kind, as the perch, have but a small degree of heat, about one degree, above the medium in which they live. This heat seems to be at the same degree above the medium in which they are, of whatever temperature that may be. Thus the flounder, cod fish, and haddock, were not found above a degree warmer than the salt water in which they were swimming, even when that was so low as  $41^{\circ}$ ; trouts were at  $62^{\circ}$ , when the water they were swimming in was  $61^{\circ}$ . Snails have a heat about  $2^{\circ}$  higher than the air. Frogs and land tortoises have a power of generating heat something stronger, about  $5^{\circ}$  higher than the air in which they breathe. This is likewise the case with sea tortoises, toads, vipers, and all the serpent kind. All the insect tribe as far as is hitherto known, excepting bees, belong to the cold animals. Caterpillars are but a degree or two above the air they live in. Bees



belong to the hot animals, and a thermometer placed in a beehive stands at  $97^{\circ}$ , the human heat.

By far the greatest part of cold animals live in water, which they can very well do without having any great power of generating heat, as water is never colder than  $32^{\circ}$ , and seldom acquires a heat above  $60^{\circ}$  or  $70^{\circ}$ . Hence they stand in less need of a strong power of generating heat, than the animals that live in the air, which is liable to a much greater alteration of temperature than water is. The quantity of heat generated by cold animals is not very great on ordinary occasions.

But in some particular cases, when the heat of the medium in which they live is in danger of being diminished below a certain pitch, then the quantity of heat generated in a given time is much greater. The following experiment proves this. Two phials of the same shape and size with an equal quantity of water in each were immersed in a mixture of snow and salt in a room whose temperature was  $24^{\circ}$ . Into one of them was put a small fish; the other served as a standard. The phial in which there was nothing but water froze in a little time. The phial with the fish soon had a ring of ice formed at the sides of the phial, but a quantity of water immediately in contact with the fish could not be frozen in less than half an hour after the water in the other phial was frozen, though the quantity of the mixture of snow and salt was very considerable. The heat generated by this little animal during this time

must have been very great, to have resisted the freezing power of so large a mixture. After all, the water could not be frozen, till the fish, by the violence of its struggles, in consequence of its tail being fixed in the ice, had killed itself. The heat of this fish was about a degree above the heat of the water at  $34^{\circ}$  in which it was swimming.

All the cold animals are scalded to death with a degree of heat, which would not hurt the hot animals. But most of them can bear a much greater degree of cold without injury. Fishes, as they breathe water as we do air, cannot live when the water freezes by the cold. All the other cold animals bear the winter without injury. But they generally retire into lurking places, so as to be affected with it as little as possible. The insect tribe, though the most tender and delicate of all animals, can bear, unhurt, the greatest degree of cold. They are preserved in the coldest seasons, with little other shelter than the leaves, and the rugged barks of trees and shrubs, holes in walls, or a very slight covering of earth; and some of them expose themselves perfectly undefended. Dr. Boerhaave observes that in the bitter winters of 1709 and 1729, insect eggs and insects, in the state of nymphæ, outlived the cold, that was intolerable to the livelier animals. Even in the coldest climate of this globe, there are innumerable swarms of flies, whose eggs and aureliæ must have survived the winter cold. The French mathematicians in Lapland were greatly pestered

with these animals, though the cold of their winter was  $33^{\circ}$  below the beginning of Fahrenheit's scale. Reaumur observes, that very young caterpillars bear without injury a degree of cold equal to  $4^{\circ}$ .

The motion of the heart of the cold animals is much slower than that of the hot animals, and during the severe cold of the winter it does not move at all. Dr. Harvey has several facts to this purpose. In snails, the heart, which beats very manifestly in summer, was found to be perfectly at rest in winter. The same fact appeared in many of the fly tribe and others, which pass the winter in a sleepy or insensible state. These animals seem to have a kind of life more of a vegetable than animal nature. Of this sort are the viper and serpent tribe, frogs, toads, and tortoises; even the bat, which is naturally a hot animal, passes the winter in this state of sleep or insensibility, and during this period is as cold as the surrounding medium, and its heart is perfectly at rest. Yet these animals, in this situation, scarcely to be distinguished from death, are immediately restored to their original state of activity and warmth, by being kept for some time in a warm place.

Though the warm animals never have their blood much warmer than its ordinary temperature in health, and are incapable of bearing such increased heat, they can, however, bear degrees of cold far below it without danger. Boyle gives many instances of animals that have become stiff with cold, or which have been buried in lumps of ice, that by being exposed to a warmer

air, have been again restored to their life and senses. Partial freezing of the nose, hands, and feet of the human body are frequent. Hildanus gives an instance of men so frozen by the mists, that when plunged into cold water they contracted a cover of ice over the whole body, and therefore must have been of a temperature considerably below 32. Yet even in this terrible state people have been recovered to life again by a method of treatment well understood in these intemperate climates. The great degrees of cold, which the different parts of the human body frequently suffer, are equal to, or even below what is necessary for freezing the blood. It may, therefore be imagined, that in this case it would be impossible to restore the body to its original state, especially, as it was asserted in a former part of this essay, that if the blood was once coagulated, it was impossible by exposing it to heat, ever to make it regain its original fluidity. But though this is the case upon exposing blood to the open air to cool slowly and gradually, and has hitherto been supposed to happen in every case, yet if the blood is exposed to a great degree of cold, and suddenly cooled, something different happens. If the blood is cooled to a great degree, and suddenly frozen, it can be preserved in that state for any length of time without becoming putrid, and upon exposing it to a gradually increasing heat, it becomes as fluid as when it ran from the vein of the animal; and after this, if it is allowed to cool slowly in the

open air, it will coagulate like blood fresh drawn, and then cannot be again restored to its natural fluidity by heat. Now in all these partial freezings of the human body, the blood must be frozen before it can coagulate; for the cold is too suddenly applied to give it time to coagulate before it freezes, and therefore the blood in the vessels regains its original fluidity, when the frozen member is restored to warmth.

It appears from these observations, that though the nervous influence is destroyed by much less heat than what is necessary for coagulating the blood, that it suffers very little by a degree of cold, which easily freezes that fluid. For in every case where a frozen member of the human body has been restored to its natural state, it is evident that the nervous energy remains, otherwise the restoring the fluidity of the blood would be of no consequence; the part would infallibly mortify; for the blood vessels, in this case, deprived of their sensibility, would allow the blood to stagnate for ever in them. However, though the nervous influence can be restored, when the nerves have suffered a degree of cold something below that of freezing blood, yet if the cold is much below this, they are entirely destroyed, and cannot be again restored to their sensibility at all. Hence in many cases frozen hands and feet become gangrenous in spite of every method of treatment. When a member has been frost-bit, it is necessary to restore the heat to it very gradually. If it is put



immediately into warm water, it infallibly mortifies. Hence in cold climates, where accidents of this kind are frequent, they put the frozen limb into a mixture of snow and water, or rub it with snow for a considerable time. All sudden alterations of heat are dangerous to the bodies of animals. The difference between the heat of the frozen member and that of water, which we could call hot, is 70 or 80 degrees; and the state of the nerves at that time would probably make water at 70° or 80° produce the same effect as scalding water would have on them on ordinary occasions.

The animals which pass the winter in a state of insensibility are such, probably, whose blood is frozen with a much less degree of cold, than that necessary for destroying the nerves. Hence they pass the winter without requiring nourishment, and without any waste of substance or danger of putrefaction. When the returning sun in the spring restores the life of all the vegetable kingdom, the blood of these sleeping animals regains its fluidity; the animal is restored to its state of activity and vigour, in the same manner as a frost-bit limb is restored to life by a gradual increase of heat. If the winter has been very severe, the cold may be so great as to destroy their nervous influence altogether, so that the animals cannot be restored to life by the returning warmth. In order to prevent accidents of this kind, they retire into holes in the earth, where they are in less danger of suffering too great a degree



of cold. Hence, if an animal, whose blood froze before its nerves were destroyed, were exposed to a sudden cold, it might be reduced artificially to this state of insensibility, like the animals, which pass the winter in a state of inaction, and might be kept for any time in this state, and restored to life whenever we pleased, by a gradual application of heat. I once assisted at a trial of this kind on a fish; but we could not freeze it suddenly, on account of the power of generating heat, which the animal was found to have, as already mentioned, so that it was killed by its own struggles before it could be frozen. A swallow would be more proper for this trial. Every body knows, that flies, which are apparently dead after a severe frost, will recover in the sun beams, after being in this state of insensibility for several days.

This account of the matter, and these experiments, render this opinion about animals that die in winter very probable. Future experiments, however, will throw more light on this subject.

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## ESSAY III.

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### *ON EVAPORATION.*

A. D. 1783.

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**BODIES** are converted into vapour chiefly by means of heat. They differ, however, very much with regard to the degree necessary for this purpose. In general we find that those bodies which remain fluid in the ordinary heat of our weather, are more easily converted into vapour, than those that are solid, and if any of the commonly fluid bodies are accidentally frozen, heat first makes them fluid, and afterwards changes them into vapour. In this state they remain until the heat be diminished below a certain degree, and then they return to their fluid or solid state according to the degree to which the heat is reduced. There are, indeed, a few solid bodies, which are immediately converted into vapour without becoming fluid. But the vapour produced from these bodies is in no respect different from that of

others at least with regard to its general qualities. The vapour of all bodies is elastic, and its elasticity is inversely as the space which it occupies.

The production of vapour from fluids is attended sometimes with a particular commotion, which cannot take place in solids. This boiling in fluids may be produced at very different degrees in the scale of heat. These variations are found to depend on the situation of the body with regard to the pressure of the atmosphere. If the pressure is increased, the heat necessary to make the fluid boil is greater, and the pressure being diminished, is accompanied with a diminution of the boiling heat. These circumstances have been long established by the most undisputed experiments, and it is now universally agreed that the pressure of the atmosphere, and the boiling heat of fluids, are in a constant ratio to each other, and one of them being given, the other may be immediately discovered.

Evaporation has been divided into sensible and spontaneous. But these are the same, differing only in degree. It is not solely at the boiling point that fluids are converted into vapour. They also evaporate in a degree of heat far below it. But the quantity of vapour produced from a body in a given time, is greater when it does, than when it does not boil. The vapour is also in the first case very elastic and difficult to be confined excepting in vessels of great strength; whereas the vapour produced below the boiling point has but little elasticity, and may

easily be confined in the weakest vessels. Philosophers have not yet discovered the least degree of heat by which a body may be converted into vapour. We find in the case of water that it is converted into vapour even in the form of ice without passing into a fluid state. If we breathe on a polished plate of metal that has been exposed to the air during an intense frost, a film of hoar frost immediately congeals upon it; but in a short time this disappears, though the air be much colder than frost, and without any drops of water being seen upon its surface. But evaporation in this case proceeds very slowly, in some measure depends on accidental circumstances, and varies at different times while the external temperature is the same. The dryness or moisture of the air seems to have considerable effect upon it.

As water forms a very great part of this earth, we have opportunities of making more observations on the evaporation of that fluid. We shall therefore confine our remarks chiefly to it. From the observations, which every body has made, this fluid appears to evaporate more quickly the greater the degree of heat to which it is exposed. We perceive that evaporation is greater in hot than in cold weather, that artificial heat produces vapour in still greater quantity, that the more we increase the heat the more quickly the water evaporates. These things are so obvious that one might have omitted them, were it not that a very ingenious and cele

beated writer has lately disputed them, and advanced an opinion that ought not to be passed over without examination. The author I mean is Mr. Bergman, chemical professor at Upsal. This gentleman, in a late volume of his *Opuscula* says, that almost every operation in chemistry has a certain degree of heat, as it were, allotted for it, and in which it may be performed in a less time than by either a greater or lesser degree of heat; that water is subject to this rule; that it is converted most readily into vapour at  $150^{\circ}$  of the Swedish thermometer, which is equal to  $302^{\circ}$  of Fahrenheit; that this point in the scale of heat may, with regard to water, be considered as similar to a point of contrary flexure in a curve on each side of which the ordinates diminish gradually till they vanish.

According to this doctrine the hotter the fire is above 302, the less water is dissipated, and we are led to conclude that water intensely hot is not more disposed to become vapour than in the severest frost, and that heating water red-hot in a pistol-barrel, which has hitherto been reckoned a hazardous experiment, and which was thought no small proof of courage in the gentlemen who first tried it, is, on the contrary, very safe; at least the great danger is at  $302^{\circ}$ , but beyond this it gradually diminishes, and at the red heat at which the pistol bursts, it should have vanished altogether. I wish Mr. Bergman had mentioned the experiment on which he founds this opinion. On most other occasions he has not taken



tip opinions rashly, but has proceeded with great caution and attention, and appears to have perfectly understood the principles of the Baconian philosophy. In the present instance, however, his opinion seems to be so contrary to common sense and observation, that few will assent to it, but most persons be apt to say that it deserves no serious consideration. It is similar to that notion which has been lately stated to prove that none of the stars are much larger than our sun. It is affirmed that if one of the stars was five hundred or a thousand times larger than the sun, and placed at the ordinary distance we commonly suppose the fixed stars to be, we could not see it, because before its light could reach us the whole of it would have fallen back to the original body by the force of attraction. We must not, therefore, expect to see a luminous body merely because it is large, for beyond a certain size, the larger it is the less probability we have of seeing it. Our only chance is, that it should be small.

It is not easy to find out in what manner Mr. Bergman could prove by experiment, that water evaporated faster at  $302^{\circ}$  than at any other degree. For water flies off in vapour long before it can be heated to this point in the open air. If we apply artificial pressure to the surface of water, it may acquire this degree of heat, but it does not evaporate. If we apply pressure till the water heats to  $302^{\circ}$ , and then remove the pressure, the water evaporates in part, but more of it, I find, if the pressure is continued

till the heat be raised to 500. These things are opposite to Mr. Bergman's notion, who in this case seems to insinuate that heat is not the cause of evaporation. All experiments prove this to be without foundation, and we may still admit, that the greater the degree of heat to which water is exposed, the more quickly it will evaporate; and I believe we may also admit that the larger a body is, the more easily it will be seen.

Water when converted into vapour increases in its size about 1800 times, but this is strictly applicable only to vapour produced at the boiling point. It is not easy to ascertain the expansion of water when converted into vapour in lower temperatures, though it is probably not much less than this. Many authors, among whom is Desaguliers, make the expansion above 14000 times. But the experiment from which he has taken this number does not admit of an expansion much above the tenth part of it. The vapour of water is therefore much lighter than air, which is only between 800 and 850 times lighter than water. The specific gravity of steam to that of air is as 1800 to 800 or as 9 : 4.

If vapour could retain its form independent of external cold, it would rise till the density of the air was diminished in the proportion of 9 : 4. At this place the barometer would stand at 13.33 inches, and the perpendicular height above the level of the sea would be something less than four miles.

But as the vapour rises, the pressure on it would

diminish, and at the height of four miles being pressed with but one-half of the force, it would occupy double the space it did at the surface of the earth. It would still, therefore, continue to rise, and the same cause continuing to operate, it would reach the top of the atmosphere.

The whole vapour, however, never reaches to the height even of four miles perpendicular. A considerable part of it is condensed before it can rise to that height, and the part which does not condense is no longer pure vapour, such as we have supposed it at first. Though a great part of the vapour of water is condensed before it rises four miles perpendicular, a part of it ascends near as high before it condenses. This is evident from the snow on the tops of hills. The summit of the Andes is covered with snow though under the equator, and above a certain height the snow constantly lies. This height the French academicians call the level of the snow.

The vapour of water then does not always return immediately to the state of water upon being cooled. A part of it mixes with the air, and is dissolved by it, and may then rise to a great height in the atmosphere. When we stand near the boiler of a fire-engine, and the machine stops, and the steam is let out, a white cloud of vapour issues with great violence, but after rising a little way it disappears. It does not, however, immediately fall down again in the form of water, though from the temperature

of the external air it must instantly be cooled, far below the boiling point. This must be owing to the vapour being dissolved in the air. For, if we hold any cold body in this steam, and retard its mixture with the air, drops of water are instantly formed on it. When hot water is put under the receiver of an air-pump, and the air taken out, the water evaporates, but this vapour immediately condenses upon cooling, and little or none will be found in the receiver, when it has acquired the temperature of the external air. The air at all times contains a quantity of vapour dissolved, and may be made to part with it, either by cold, or by exposing bodies to it which have a strong attraction for water, and which will soon lay hold of the vapour, unite with it, and be dissolved by it. Air, which has dissolved vapour, is lighter than pure air. Equal parts of air and vapour should produce a compound, whose density to common air should be as 8 : 13. Suppose the air to be free from all vapour of water, when the barometer stands at 30 inches, if at any time the air was mixed with an equal quantity of watery vapour, the barometer should fall to 19 inches. In our climate, the barometer seldom falls below 28 inches; which, independent of all effects from accidental circumstances, of rarefaction from wind and other causes, would give the proportion of vapour mixed with air, when greatest in our climate, to amount to a tenth part. In this case, a gallon of air should be found to contain 3.64 grains of

water, about three and a half grains. This, I believe, is less than may be sometimes separated from it, both by cooling it, and other means. In a mixture of equal parts of air and vapour, the gallon should be found to contain from 16 to 17 grains of water. I have here supposed the specific gravity of air to be  $\frac{1}{8}$  of that of water, and the heat, the mean heat of this earth, to wit,  $56^{\circ}$ .

I have taken it for granted that air in some cases has no vapour mixed with it; this, however, never happens. I have also assumed, that the vapour and air after mixture occupy the same space which they did before mixture, which is also not exactly true. These will affect the conclusions a little, but not so much as might at first sight be imagined. The rising and falling of the barometer depends more commonly on the quantity of vapour mixed with the air, than upon winds or any other causes that have been assigned.

When vapour is produced suddenly from a fluid it becomes colder. It has long been observed in China, that wet cloths exposed to the air are colder than the temperature of the air, as long as they retain any moisture. Accordingly, in that country, rooms and galleries formed of canvas are kept constantly wet to give coolness to the air within. They also cool jars containing water in the same manner, and upon this principle also, they are enabled to make ice in places where the temperature of the air is seldom so low as the freezing point. Though

the Chinese seem to have first observed that the evaporation of water produced cold, they do not seem to have remarked it in other fluids. It is, however, now known, that all fluids when they evaporate produce cold, and that the cold produced is in proportion to the volatility of the fluid. By moistening bodies with ether, we can cool them below the freezing point in the hottest day in summer. When water is exposed to an intense heat in a close vessel, it becomes much colder immediately upon opening it, and allowing the vapour to issue. But this vapour is not hotter than the common boiling point of water. The quantity of heat which disappears in this case would have heated the fluid 800°, as I have found by experiment some years ago. One may naturally ask, is the production of cold the consequence or the cause of the evaporation? But we know too little about vapour to be able to give a satisfactory answer to this question. It may be owing to the one or both, or to some other cause unknown to us. The only thing we are sure of is, that the production of cold, and the conversion of a body into vapour, always go together. Another circumstance in vapour is, that it continues ever after to be more difficultly heated than in the state of water, and from this, the cold produced in evaporation may be accounted for, as I mentioned in a former essay.

Great alterations in the heat of the weather may be expected to arise from the causes just stated. For



vapour is always forming or condensing, and according as the one or other prevails, the heat produced exceeds the cold, and the contrary. The quantity of vapour formed by the ordinary heat of the weather far exceeds that produced by artificial fire. It has been found that the water which evaporates from an acre of new mown meadow in twelve hours amounts to sixteen hundred or two thousand gallons, and when the sun's rays acted with great force, by being reflected from a brick wall, two thousand eight hundred gallons were evaporated from an acre. In the torrid zone it is not improbable, that during the twenty-four hours in the rainy season, the water evaporated from an acre will be double this quantity, or will amount to five or six thousand gallons. To this must be added the great quantity of smoke which arises from the inflammation of combustible bodies and which consists chiefly of water.

The cause of the formation of vapour has been variously assigned. Philosophers have been much interested in the question, but none of the theories are quite satisfactory. It is not my intention to mention them all. Two have been reckoned more probable than the rest, and deserve on that account to be noticed. The first is, that water is converted into vapour by being dissolved in the air, in the same manner as a salt dissolves in water. The other opinion is, that the water is converted into vapour solely by heat. The first of these is apparently well supported by many circumstances. Such are,

is the cause of spontaneous vapour. Evaporation seems to be the joint effect of both heat and air. There seems to be two ways in which this may be effected, either by the air acting on the water in its ordinary form, and dissolving it immediately, or by the air combining with the vapour of the water, and preventing it from condensing and falling down in rain so fast as it would otherwise have done. The vapour which heat raises is instantly united with the air, and by that means prevented from condensing. The quantity of vapour which the air is capable of dissolving depends on two circumstances, its heat, and the moisture already combined with it. If it is warm, every thing else being equal, it will unite with more vapour than when cold. But warm air may already be saturated with vapour: it will then take up no more, and the vapour then raised by heat will condense again into water. If the air is very cold, it may sometimes be capable of combining with more vapour than air apparently warmer, because air, when cooled, necessarily parts with some of the vapour it had dissolved. If, after this, it should be suddenly heated, though but for a few degrees, it will dissolve vapour very fast: for the cold to which it had lately been exposed, having thrown out of it a great deal of vapour, it is, though still very cold, disposed to attract it very greedily, and much more quickly than hotter air loaded with vapour. The vapour in our breath is visible in a cold day, because the air is not then able to dissolve

it; but it is not visible in a hot day, unless the air be already saturated without vapour, which it sometimes is.

It has been found by experiment, that removing the vapour which stagnates over a hot fluid increases the evaporation, even when the quantity of vapour is so great that the air has not time to dissolve it. A stream of air, though saturated with vapour, may in some cases promote evaporations, and the drying effect even of moist winds may probably depend upon this. Perhaps also air dissolves water, or even ice, in their ordinary form, though this is not so clearly proved by experiment, and most of the phenomena of evaporation may be explained without it. At the same time, we have instances of bodies differing almost as much in density as air and water, which dissolve each other, and the principal difficulty lies in conceiving, how air, by dissolving a dense body like water, should form a compound specifically lighter than it is when pure. If this be really the case, it seems to be necessary to admit, that water is converted into vapour previously to its combining with the air, and, if I remember right, ice in vacuo exposed during an intense frost, actually diminished in weight, and evaporated. This is not so surprising, when we consider that mercury, whose vaporific point is  $672^{\circ}$ , evaporates in vacuo below  $212$  from Colonel Roy's experiments. If the vaporific point of ice in vacuo be as far below the boiling point of water in the open air, ice

should evaporate in vacuo at 248 below the zero of Fahrenheit.

Most authors seem to believe, that air combined with vapour is heavier than dry air. But if this were the case, why does it rise so fast through the rest of the air. It is not easily conceived that this is produced by the chemical attraction between dry air and air loaded with vapour, for the ascent of the moist air should in this case be slow. Neither will wind nor agitation of the air account for it; for vapour rises when there is no wind. Air that has been heated through the day, and which cools soon after sunset, deposits in the form of dew, some of the vapour which it cannot then keep dissolved; though sometimes dew is produced by the vapour issuing from plants, &c. which is condensed from the air being too cold to dissolve it. When the air near the surface of the earth has dissolved vapour, and a cold wind immediately rises, the air containing the vapour is cooled, the further solution of vapour is prevented, a part of that which was dissolved separates, and an opacity in the air is produced from the watery vapour condensing, and a fog or mist arises. Sometimes this happens only in the morning, and soon after sun-rise the air being heated redissolves the vapour, and the day clears up.

The air considerably higher than the surface of the earth, is colder than that below. When warm air which has dissolved vapour rises to a certain height, it will cool, and deposit some of the vapour

which it had dissolved, and this will appear in the form of clouds. The height at which this happens, depends on the heat of the weather, and hence the clouds are generally higher in summer than in winter; in a hot than in a cold day. Hence hills, the tops of which are colder than the valleys, and nearer to the place where the separation of vapour from the air by cold takes place, attract a great deal of the vapour which condenses; and hence the moisture on hills, and the springs of water that are there to be met with. If the mountain is very high, the air will have parted with most of its vapour before it reaches the top, and hence the summit of very high hills is often clear, when there are clouds on the lower parts. It is, perhaps, also owing to this, that, on the tops of very high mountains, the air is very dry, as De Luc observed it to be on some of those in the neighbourhood of Geneva.

A considerable part of the vapour dissolved in the air would separate as it rises, independent of cold. When we rarefy the air in the receiver of an air pump, the operation is always attended with a mistiness and opacity in it, which is owing to the vapour being incapable of rarefaction beyond a certain degree, without a part of it condensing and furnishing heat to the increased capacity of the remainder: otherwise it could not exist in this rarefied state, as I have endeavoured to shew in a following essay, on the reason why the falling of the barometer is generally attended with snow or rain.

In a few instances, vapour of water, and even of other bodies, may be found still higher than the tops of the highest mountains; and meteors, if they are not solid bodies, are either produced from vapour rising to a prodigious height in the atmosphere, or are a consequence of that vapour. We do not sufficiently understand the nature of these bodies, but from the attention that is for the future likely to be bestowed upon them, we have reason to hope that we shall soon become better acquainted with them. The vapour arising from some substances will not combine at all, or only in a small quantity, with the air. The substance known by the name of inflammable air is of this kind, and as it is much rarer than common air, it may rise to a very great height in the atmosphere. We do not yet know exactly the height to which meteors rise, though the last one, it was imagined, was not very high. If these bodies are really produced by electricity very high in the air, this may arise from the vapour of bodies getting into a different state, and condensing into water or snow. By some late experiments, it has been found, that water converted into vapour by a fire whose electrical communication is cut off by setting it on a glass plate that was cooled on one side, gives an electrical spark every time water is thrown upon the coals. The vapour that issues must therefore, in its natural state, contain either less or more electricity than the water from which it was produced, and after rising to such a great



height as to be out of the reach of any conducting substance, a part of it may very suddenly condense into water ; the whole would become charged with electricity either positive or negative. The electricity in the clouds, it is not improbable, may arise from vapour condensing into rain, or freezing into snow or hail, and the clouds being plus or minus, may depend on the condensation being either from vapour into rain, or vapour into snow. We have reason to expect electrical appearances wherever the vapour of bodies reaches, and this electricity arising from a partial condensation of vapour or separation of it from the air, may greatly affect the part of the vapour which still remains combined with air, and dispose a part of it to separate sooner, and the rest to rise to a greater height than it otherwise would have done. It seems to be also extremely probable, that the ascent of vapour and its disposition to separate from the air, depend much on the state of the atmosphere with regard to electricity. But this I shall not at present discuss.

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## ESSAY IV.

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### ON RAIN.

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THE greatest part of the water which is evaporated from the surface of this earth by the heat of the sun, descends again in rain. The rest falls in the form of snow, hail, and dew. Rain, snow, and hail fall in great quantities at one time, and not at all in another; but there are few days in which there is no dew. The quantity of rain greatly exceeds, at least in temperate climates, the quantity of snow and hail, and may, therefore, be considered as the principal cause of that constant descent of water in fountains and rivers from the higher to the lower parts of this globe, upon which the existence of vegetables depends, and by whose means this earth becomes a healthful as well as delightful habitation to man.

The quantity of rain is different at different seasons of the year, in different climates, and in different

situations in the same climate. More falls near high mountains than in the same latitude in a level country. Many instruments have been contrived, by which the quantity of rain may be measured with tolerable accuracy. By means of observations made with these instruments, it has been found, that the quantity of rain in one place is often double that at another, though the distance between them be but small. The quantity of rain in some parts of Lancashire is nearly double what falls in London. The quantity of rain in London and Paris is nearly the same, though the distance of those places is not less than that between Rippon in Lancashire and London. The quantity of rain at Pisa in Italy, a place situated  $40^{\circ} 37'$  W. or  $7^{\circ}$  South of London, is much greater than at London or Paris, and equals that in Lancashire. This is the more surprising, when we consider, that Pisa is but five miles from the sea. It is in general agreed, that there is more rain in the temperate than the torrid zone, more in a high than a low latitude. In certain situations, however, this does not take place. The neighbourhood of high land will make a place much more rainy than it otherwise would have been. Every person has observed that it frequently rains on the tops of hills, while it is fair in the low country. When there is more rain in a place near the equator than in another at a considerable distance from it, we generally find that high land near the most equatorial place is the cause of the exception to the general rule. Though,

however, the quantity of rain near the equator may, from neighbouring high land, equal or even exceed that in places near the poles, yet the number of rainy days is greater in the temperate than in the torrid zone; and greater in those parts of the temperate zone which are farthest from the equator. In many places near the equator, the quantity of rain is as great as any where in Britain, but the whole of this rain falls in a few days, whereas with us, it rains 70 or 80 days of the year at a medium: This difference in the time, in which a given quantity of rain falls, will occasion a great difference in its effects upon the soil of the two countries. In the one country, the rain falls in torrents. The greatest part of it runs with violence along the surface of the ground, destroying vegetables and animals, carrying the soil along with it, and but little gets time to sink into the ground, so as to serve for the refreshment and growth of vegetables. On the contrary, where rain falls gradually, a great part of it penetrates the soil, and serves as nourishment for vegetables; the rest appears again at a considerable distance in the form of springs and fountains.

We cannot judge of the moisture of a country by being barely told how much rain falls in a year. A country may be very dry, though more rain falls in it in a year than in any part of Great Britain; yet the whole of this island is to be reckoned moist. Were it to rain with the violence it does in some places near the equator, 20 or 22 inches of rain

might fall in ten days. But though this quantity of rain be considerable, if there be no more, there would be 355 dry days and 10 wet ones that year; the air would be in general dry, and the climate instead of being reckoned moist would be well entitled to the character of dryness. Twenty, or 22 inches is the quantity of rain nearly in Great Britain in a year; but a part of this falling almost every day, gives a great degree of moisture to the air; a great part is absorbed by the leaves of plants, and most of the rest sinks into the earth. The climate is, therefore, well entitled to the character of a moist one.

As the time in which a given quantity of rain falls, has such different effects, it would seem proper in every register of rain to mention particularly, not only the quantity, but how many days, hours, and minutes it continued to rain. If one knew how much rain penetrated the earth in a given time, a rule might be made from which its effects in moistening the earth might be discovered. The effect within certain limits would be in the compound ratio of the quantity of rain and time it rained. Thus, if ten inches of rain fell in one place in a week, and it rains in another ten weeks in half a year, and the quantity of rain is also ten inches, one surely could not be very far wrong in saying, that the effect of the rain in moistening the soil and air of these two places, would be as 10 to 100, or as 1 to 10. Though in this case, we have supposed the quantity of rain to be exactly equal in the two places, yet ten times



the quantity of rain will penetrate the soil, where it rains ten weeks, than penetrate it where it rains but one week. Where it rains ten inches in a week, the greatest part of it will immediately run off the soil towards rivers and other superficial streams of water, and swell them soon to a destructive size. Very little will sink into the soil, and but little will be evaporated. The air, therefore, will in general be dry. Where it rains ten inches in ten weeks, very little will run off the soil, or get immediately into rivers. The greatest part will sink into the earth, and either enter into the composition of vegetables and minerals, or, after running a great way below the surface, will burst out in the form of springs and fountains, whose constant stream will supply the rivers gradually with clear water, make them glide gently, smoothly, and uniformly along; and neither raise them to such a size as to become turbulent and destructive torrents, nor furnish a supply so scanty that their channels shall become dry.

If one had been disposed to be a little more accurate, some allowance ought to have been made on account of the greater degree of dryness which the soil would acquire in the place where it rained but a week in half a year. We may easily suppose that the soil in this dry climate will imbibe moisture more greedily, and that much more rain would sink into it in the same time. But though this be granted, the effect will evidently be greatest, when

the rain begins to fall, and will become very little the moment the surface of the ground is moistened. After every deduction made on this account, it must still be admitted, that the effect of the rain in moistening the soil and air would at least be eight or nine times greater in the places where it rains ten weeks in half a year, than where it rains only one week.

It would therefore be proper to pay some attention to the time, as well as to the quantity of rain, if we wish to have a proper notion of the moisture of any country. Though this rule, however, may be useful, it is by no means meant that it is perfectly accurate. For there is evidently a certain velocity, with which if rain fall, the whole of it will be absorbed by the earth. If the rain fall with still less velocity in another place, the whole would also be absorbed. If the effect of rain upon the character of a climate were in the compound ratio of the quantity and time it takes to fall, the last of these places should be much more moist than the first; because the quantity of rain multiplied by the time gives the greatest product. In the place where it rains for the longest time, indeed, more of the rain will be evaporated, and the air may be moister for a while. But independent of this circumstance, the effect of the rain in moistening the earth in both cases is exactly the same, and is proportional to the quantity of rain. If one could discover how much rain penetrates the soil in a given time, or what

were the differences in this respect in different soils; it would not be difficult to adapt the rule to all cases. But it is not meant at present to suggest any more, than that something besides the quantity of rain in a country must be known, before the degree of moisture in the soil and air of that country, or its disposition to rear plants, can be discovered. Besides the quantity of rain and the time it falls, the quantity of dew and snow may have influence on the moisture of a country. The quantity of snow combined with the time it takes to melt will have effects similar to violent or gentle rain.

Places where there is seldom either rain or snow may be sufficiently moist by being subject to inundations, from the swelling of rivers by rain, which has fallen in another region; or they may be supplied with moisture in a great measure by dew. Egypt is a country, in which according to authors, it seldom or never rains. It is, however, very fertile, and the fertility depends on the soil being partly soaked by the annual overflowing of the Nile; the rest is supplied with moisture from dews, or from the rain which has sunk into the earth in the higher parts of the country.

The rain which falls in a year is not equally distributed throughout the year. In the colder parts of the temperate zone, more rain falls in winter than in summer; and in winter, the month of November and end of October, and the month of Ja-

January and beginning of February, are wetter than the rest of the winter months. In almost every part of this earth a great deal of rain falls near the vernal and near the autumnal equinoxes. It is not easy to account for this unequal distribution of rain. Many have endeavoured to explain it, but few have succeeded. It may perhaps appear surprising, that the cause of so common a thing as rain should have been so long undiscovered, and it might perhaps be considered as a proof of the small progress which natural philosophy has hitherto made, were it not that we frequently find in other things that the most common are the most difficult to explain.

The ancient account of rain is, that the vapour in the clouds was compressed, and its particles driven together by the wind, in consequence of which it immediately fell down in rain. To this Lucretius adds, that the heat of the sun acting upon the clouds melts rain out of them, and produces it in the same manner as wax is melted before the fire. Many of the modern solutions are not much more satisfactory. It is very generally admitted that the condensation of vapour, which is raised from water by the sun, is the immediate cause of rain. The only objection to this opinion is, that in some places more rain falls in a year than is evaporated by the sun in the same time. This, however, does not always happen, and the quantity of rain is as often smaller as greater than the water evaporated. The

difference in both cases is easily accounted for. The lightness of vapour renders it easy for winds to transport it far from the place of its formation, and accumulate it in one part and diminish it in another.

But though it be admitted that rain arises from the condensation of vapour, still it is not easy to understand why it falls at one time and not at another. We should rather expect that vapour should be constantly condensing, and therefore that rain should fall every day ; for during every day a considerable quantity of water is evaporated and carried immediately to a considerable height above the surface of the earth, where, we know, there is always a great degree of cold. This should condense the vapour, and make it constantly descend in rain. Or if the vapour, as seems most probable, be dissolved in air as fast as it is formed, this air ought, as soon as it is cooled, to part with a quantity of that water which the heat had enabled it to dissolve. We should therefore always have rain when air containing vapour is cooled, and this must take place every day. This does not happen, however, so far as is known, in any climate. We find indeed that dew falls almost every day every where. The condensation of vapour by cold may therefore, perhaps, be the cause of dew, at least of that part of it which falls. But part of what is called dew does not arise from condensed vapours, or from a separation of water from air in consequence of cold, but is formed from

the condensation of the watery vapours, which plants perspire. This vapour is not visible in the day time, because the air is then heated, and instantly dissolves it. But during the night when the air is generally colder, many plants seem to smoke, and a part of this vapour condenses into drops of water. If the night be not colder than the day no condensation will take place. Let us suppose that all dew falls in consequence of the separation of vapour from air by cold: if the night be warmer than the day, no dew ought to fall; if the night is colder at the beginning and warmer towards the end, the dew produced at the beginning of the night by cold may be evaporated again before morning; or the heated air, which kept the vapour dissolved, and from which dew ought to have been formed by cold, may be carried to a great distance by the wind shifting and becoming colder and drier.

When wind is very cold, little moisture will be evaporated; and besides, in this case there is less difference between the heat of the day and that of the night; and therefore during cold winds, every thing else being equal, there should be least dew. Even during cold winds there may be dew, if the heat of the day and night differ considerably. There is generally much dew when the wind blows from the south, especially near the equinoxes, when the difference between the heat of the day and night is greatest. This is often most evident in autumn. In summer the heat of the night differs less from that



of the day, and the dews are in smaller quantity. It may even happen during the continuance of warm winds that there shall be no dew, provided the night be as warm, or warmer than the day.

It has been long observed, that during north-east, or east winds, there is little dew in this country. Aristotle observes, the same was the case in Greece, and that the south wind always produced the greatest dews. He owns indeed that in Pontus the north-east wind was often accompanied with dew, which he is not surpris'd at, because, according to him, the vapour rais'd by the north-east wind is greater than that produced even by the south winds, as is evidently proved by observing that wells smoke most during a north wind.

But though the mere change of temperature be sufficient to account for dew, it will not explain why rain falls only at certain times. Some have imagined that the principal cause of rain arises from an effect which the higher parts of the earth have upon air loaded with moisture. It is a common observation that hilly countries have more rain than places where the ground is almost level. The tops of mountains have frequently clouds hanging over them, when none are to be seen any where else. These, it is imagined, are proofs of the high ground attracting the moisture from the air loaded with vapour, with which it is in contact. But the low or level country, being far below that part of the air to which the vapour is disposed from its

specific gravity to ascend, has little or no effect upon it. According to this doctrine, rain can only be expected in a low country, when the whole air to the very surface of the earth is loaded with vapour. But this account is not quite satisfactory. If it were well founded, we should expect that it should rain more or less every day on the tops of hills; and when it rained in one part of a level country it might be expected that it should in the whole. But neither of these are agreeable to experience. With regard to the first, we are certain, that for many days, and even weeks together, there is no rain even on the tops of the highest mountains. Neither does this theory help us to explain why in every climate some months are wetter than others. Perhaps by a little pains this theory might be so far improved, as to agree somewhat better with the phenomena; but many difficulties would still remain.

Thus it might be supposed that wind compresses the air loaded with vapour, and disposes it to part with it in the form of rain; that the hills opposing the motion of the air make it meet with more compression there than in a low country; that opposite winds blowing upon the opposite sides of a hill, and both meeting with resistance, the air on each side is greatly compressed, and made to part with the whole, or a part of the moisture which it contained. But though this is Aristotle's opinion, and has also been revived among the moderns, it does not ex-

plain why rain should fall when there is little or no wind, and why, even in hilly countries, the wind is very strong without rain, and the falling of rain is often found to moderate the violence of the wind. Another cause of rain, which has been mentioned, is thunder. It is commonly observed that rain or hail follow thunder. It is imagined by some, that the stroke which the air receives upon the bursting of a thunder cloud brings the particles of vapour in it into closer contact; they therefore run instantly together by attraction, and rain is thus produced. That something remarkable happens in such circumstances is proved by observing, that when a great number of cannon is fired, as in consequence of an engagement, the direction or violence of the wind is often altered.

To those acquainted with the effects of electricity more specious reasons will occur; such as the sudden contraction of any very light body contained in the air, in consequence of its electrical state being altered. The smoke of rosin and other bodies of a similar kind extends to a great distance upon being electrified, either positively or negatively, and suddenly contracts itself upon being restored to a natural or quiescent state. That electricity is one cause of rain, receives greater support by observing, that rain water shews itself frequently electrified in a small degree even after it has reached the earth. A late author is disposed from this to explain why

plants watered with rain water, or water which has been long exposed to the air, grow with greater vigour, and become more perfect in all their parts, than the same plants in a similar situation watered with well water. He observes, however, that this difference ceases upon electrifying the well water before using it, by setting the gardener with his watering pan in his hand upon a cake of rosin, and giving him a communication with an electrical machine in motion. But though these circumstances shew that rain, and electricity, or thunder are often joined, yet we frequently find great rains without thunder, and that thunder is generally attended only with violent showers of short duration. Electricity may have some connexion with thunder showers, and may be considered by many as accounting sufficiently for them; but it does not sufficiently explain those frequent and long continued rains, which are not accompanied with thunder, where no extraordinary marks of the electrical fluid in a very active state can be perceived.

Most persons who have endeavoured to account for rain have attempted to use only one principle for that purpose. I should imagine, that the principal mistake lies here, and that rain may be produced from several causes, and that all the causes which have been mentioned sometimes may have considerable effect in producing it. It seldom however happens, that only one of them is concerned

in it. Thunder, and wind, and high mountains, and cold have all at times a tendency to occasion rain. But all of these, excepting the last, are rather assistants than principals, and some other circumstances must be taken into account before their full effect can be ascertained. Of this kind is the alteration in the state of the air before rain falls, and which is indicated by the falling of the quicksilver in the barometer, and the effects arising from contrary winds meeting, and mutually affecting the moisture mixed with the air in each.

The barometer generally falls before rain. The pressure of the atmosphere is therefore evidently diminished, and if vapour were merely diffused through air, and not dissolved, it should be disposed to extend itself and dilate instead of contracting. Notwithstanding this we commonly find, that rarefaction of the air is accompanied with an immediate separation of the moisture contained in it. Of this we have a proof from what happens while the air is drawing out from the receiver of an air-pump. At a certain period, during the exhaustion, the receiver becomes muddy, and drops like dew in a little time settle on the sides of it, and the whole, from being opaque, becomes perfectly transparent. If after the air in the receiver has become opaque in consequence of exhaustion, the air be suddenly let in, the opacity instantly disappears. From this it is evident that rarefying of air containing vapour disposes the moisture to separate, and as

the falling of the barometer shews a similar refraction in the atmosphere, the same separation of water should take place. The reason why the diminution of pressure in air containing vapour disposes it to part with its moisture, and produce rain, I shall now endeavour to explain.

Soon after the barometer was invented, it was observed that the mercury in it did not always stand at the same height. These alterations in height were soon imagined to be connected with variations in the weather, in consequence of which barometer and weather-glass became synonymous terms. The range of the mercury in Europe at the level of the sea is about three inches; at each half inch of which is placed a word expressive of the weather it indicates. After many observations, however, it has been found that the same kind of weather does not invariably occur, when the mercury stands at the same part of the scale. But notwithstanding many occasional irregularities there is in general reason to expect fair weather, when the mercury rises, and its fall is frequently, though not always, followed by rain. Why the sinking of the mercury is commonly succeeded by rain is a question which has interested the natural philosophers. They seem in general to have admitted the method of accounting for this phenomenon, which was first proposed to them, without examining very minutely into it. This explanation appears to me to be attended with some difficulties. The principle on which this



theory depends, is that a great quantity of vapour is constantly rising from all the parts of this earth; that this vapour is mixed with the air, and that this mixture of air and vapour is heavier than pure air. As long as the barometer remains at the height at which it stood when the vapour was formed, it is suspended. When the barometer falls, and the air becomes lighter, the vapour can no longer be suspended, but descends in the form of rain or snow, according to the particular heat of the weather at the time. One great difficulty which attends this hypothesis is, that it is not easy to conceive upon these principles how the vapours can ascend to so great a height in the atmosphere. The air at the surface of the earth, it is asserted, becomes heavier than it was before, in consequence of being mixed with vapour. One would expect this mixture of air and vapour to hover on the surface of the earth, instead of rising to such a height above it, as experience shews that it does. This is perfectly contrary to what happens in other cases, where we always find that the denser body sinks instead of rising through the rarer. In the present instance the vapours are in greater quantity at a considerable height above the surface than at the surface of the earth.

In order that the vapour of water may ascend into the superior regions of the atmosphere, it is necessary to admit, that vapour, and even air mixed with vapour is lighter than pure air; and that it is so seems

evident from other circumstances besides their rising to so great a height. The vapours produced from water at the boiling point, have been found by experiment to occupy eighteen hundred times the space which the water they were produced from occupied. If the density of the vapours of water produced in the ordinary heat of the weather be the same, these vapours should rise to a great height in the atmosphere. For the density of the air at the surface of the earth in the mean temperature of the weather is to that of water as 1 : 850, and the density of vapour at the boiling point is to that of water as 1 : 1800; whence the density of air is to that of vapour, as 1800 to 850. These vapours would therefore ascend, independent of resistance and rarefaction, from diminution of pressure, till the air became of the same density with them, which it would be at that height above the surface of the earth, when the air could occupy more than double the space which it does at present, or where the mercury in the barometer would stand at about fourteen inches. Supposing it to stand at 30 inches at the level of the sea, this would be at the perpendicular height of four miles above the sea, or higher than the highest mountains in this globe. That vapours really rise to a great height, is evident from the tops of the highest hills being perpetually covered with snow. The whole of the vapours raised from water do not however rise so high, on account of their progress being stopped by the resistance which they meet with

in their passage, and their being prevented from rising perpendicularly by the force of the winds.

The air then in consequence of being mixed with vapour ought to be lighter than pure air, and experience has proved it to be so. If spontaneous vapour be not so rare as that produced at the boiling point, and if the effect of heat upon it be the same as on air, the vapour formed in the ordinary heat of our weather ought to be in density to the air as 850 : 1350. The density of the air is the same with this, when the barometer stands at twenty inches, at a height about two miles above the surface of the earth. But vapour rises higher than this, as is evident from the observations of the French academicians in the neighbourhood of Quito.

All experiments confirm the opinion, that vapour is much lighter than air. So much so that if it preserved its form, in the first case the barometer must fall to fourteen inches before it could reach the surface, and in the second to twenty inches before this could happen. But as the barometer seldom falls lower at the level of the sea than 28 inches, these vapours, if no change happened to them, would never fall down again to the earth. We find, however, that they do fall down, and how this should happen is more difficult to conceive.

One cause of the descent of vapour in the form of rain, in consequence of the barometer falling probably depends on the nature of vapour with regard to heat. It is well known that vapour contains much

more heat in it than the water from which it was produced, or, what is the same thing, that it requires more heat to heat vapour to any degree of the thermometer than to heat water to the same degree. And it has lately been found by experiment, that the difference is greatest when the difference of density is greatest: that is, there is more heat necessary to preserve vapour in the form of an elastic fluid, when it occupies a large space than when it occupies a small space. But the space which vapour occupies, as in all elastic fluids, is reciprocally as the force which compresses it. It will therefore require more heat to keep water in the form of vapour, when the barometer is low, than when it is high. That this is the case is evident from many experiments. If a quantity of vapour is included in the receiver of an air-pump, and a thermometer along with it, and a part of this vapour be drawn out by working the pump, the thermometer will sink as the quantity of vapour diminishes, and the remainder expand so as still to fill the receiver. After a certain time, however, the thermometer suddenly rises to the same degree at which it stood at first. But then the vapour divides itself into two parts, one becomes water immediately, and in doing so gives out a sufficient quantity of heat to heat the vapour to the temperature which it had at first. A part of this experiment every person has seen in the common exhaustion of an air-pump. The air in the receiver is at first perfectly transparent; when the pump is

wrought a little, it becomes muddy, and a number of drops of water condenses on the sides of the receiver.

At any given height of the barometer, there is a great quantity of vapour mixed with the air, and this mixture is lighter than pure air. This vapour is of the same temperature with the air in its neighbourhood. The moment the barometer falls, this vapour dilates itself. It must immediately either become much colder than before, or if it remains of the same temperature, it must divide itself into two parts. One part descends in rain, and the heat which this gives out serves to keep the remaining vapour of the same temperature which it had before the dilatation took place. We find accordingly, that this division of the vapour into two parts takes place principally in the higher parts of the atmosphere, and it is well known that more rain falls on the tops of hills than on an equal surface of ground lower down. Why this division of the vapour does not always follow the falling of the barometer is not altogether so easily explained, though something of this kind might be attempted with sufficient plausibility.

The last source of rain, which I shall mention, is the effect which two streams of air differing in temperature have upon the moisture, which each separately contained. This effect has been frequently observed. Every person must have noticed, that rain generally succeeds a sudden change of the wind, and that it is very seldom that rain falls in great quantity,

all experimental philosophers, have observed this. When this happens, the wind, from being northerly, becomes suddenly southerly, and the different direction of the currents is most remarkable. Before the setting in of the frost, about Christmas, and for some time after it, in the month of January, the difference of the temperature of a north and that of a south wind are perhaps at a maximum; and great quantities of rain or snow usually fall in the month of November, and at the end of January or beginning of February. This effect of two currents of air differing much in heat, is most evident at the end of a frost, and is usually accompanied with a great deal of rain. But this is not absolutely necessary. It may happen, that a thaw may be produced by a change of wind towards the south, without there being any current of colder air in an opposite direction. We need not be surprised, that such a thaw should be accompanied with little or no rain.

These observations, which every person must have made, seem to shew, that two currents of air meeting, have a considerable share in the production of rain. Dr. Hutton, of Edinburgh, has attempted to shew, that air heated, when suddenly mixed with a quantity of colder air, immediately will be attended with a separation of water. He has not confirmed it hitherto by experiment; but it seems not improbable, that the quantity of vapour, which a gallon of cold and a gallon of hot air can keep suspended, when separate, is much greater than two gallons of



air of the mean temperature can retain, and that in consequence of the mixture a quantity of vapour will separate. This we find to take place in a similar instance in water and nitre. A pound of cold water and a pound of hot water, each saturated with this salt, have been found by experiment to keep more salt dissolved when separate than two pounds of water of the mean temperature would do. Thus a pound of water at  $32^{\circ}$  will dissolve two ounces and a half of nitre: a pound of boiling water will dissolve twenty-four ounces. Two pounds of water thus keep in solution twenty-six and a half ounces of nitre. If these two solutions are mixed, a quantity of the salt separates; because two pounds of water heated to  $125^{\circ}$  will not dissolve twenty-six ounces and a half of nitre, but only ten ounces. Reasoning from analogy, which however is not much to be trusted in physical subjects, we may judge it not improbable, that something of a similar nature may take place in mixing cold air saturated with vapour, with hot air saturated with vapour, and that this may help us to explain the phenomena already mentioned. We cannot however accept of it as a complete solution, till it has been fully established by experiment.

But though the effect of contrary winds in disposing air to part with moisture were greater than is pretended, this cannot with any probability be used to account for rain in every situation. It may in part explain why a greater quantity of rain should fall in winter than in summer, and near the equinoxes

than before or after them. But it does not explain the rain, which falls in the middle of the torrid zone, where winds of different temperatures are not so common. Neither is the effect of hills in producing rain entirely accounted for by it. But this, though perhaps the great cause of rain, is often counteracted by other causes, such as the state of the air with regard to density and electricity, each of which is sufficient alone to produce rain, or to prevent it even when contrary winds favour it. The effect of hills in producing rain seems to depend in a great measure on the air about them being very cold, and this is the case even in the torrid zone. When the wind is warm, and contains much moisture, as it comes in contact with a hill, it mixes with air much colder than itself, and immediately the moisture it contains is separated. If this be held a solution of the greater quantity of rain in hilly countries, there should be least rain in them, when the wind blows from a cold quarter, such as the north-east, for a long time.

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#### NOTE BY THE EDITOR.

It is to be observed in this essay, that the quantities of rain stated to have fallen in various places are not always precisely the same as they are given in other authors. I have no doubt that the number of inches was taken upon some good grounds, probably upon the authority of experiments. I do not however know what these were. It is of the less importance to ascertain these points

since the speculations contained in this essay do not at all rest upon these numbers, which are merely occasionally quoted for the sake of illustration, and not as the ground-work of any important argument. There has been perhaps for the last twenty years less improvement in the theory of rain than in most parts of natural science. The opinions here announced will appear proportionally more like the present doctrines, and the addition offered to the causes of rain will be more likely to approach to the truth. It seems indeed to me scarcely possible to deny to the rarefaction of the air of the atmosphere, the power of producing a deposition of moisture, nearly in the manner above illustrated. Whatever opinion may be entertained of the method in which that is effected, the experiment of dilating air in the air-pump, puts it beyond the reach of controversy, that a fall of the barometer must always be accompanied with rain, unless more powerful counteracting causes interfere to prevent such precipitation.

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# ESSAY V

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## *ON THE FERTILITY OF SOILS.*

Read A. D. 1778.

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THE soil in which plants grow, consists principally of earth mixed with a small quantity of the remains of rotting vegetables. Earth forms by far the greatest part of every soil. The quantity of vegetable matter is different in different soils, and even in the same soil it varies at different times. Besides earth and vegetable matter, every soil contains a small quantity of salt, which, though sometimes it be purely saline, is very frequently of an earthy or metallic nature. The quantity of salts of every kind mixed with the soil, is always very small. If it were considerable, the fertility of the soil would be entirely destroyed. Every soil principally consists of a confused mixture of those earths which lie in regular strata immediately below it. It differs, however, much from these earths, not only by their

wanting that vegetable matter which the soil contains, but by being in a different state before and after their exposure to the air. When earth is thrown up from a considerable depth below the surface, it is long before any vegetable will grow in it. It gradually swells, and crumbles down, and after a certain time, is much more soft and spongy than it was before. This is most remarkable in the softer earths, but it may be observed even in the hardest. The change on the latter, however, is more slow and gradual. Even the scorizæ of volcanos, though little short of being in a state of vitrification, fall in time in the air, so as to allow those plants that require but little soil to grow on them. The remains of those plants mixed with what had formerly mouldered from the lava, and what has since mouldered, form a soil of a greater depth, fit for the nourishment of vegetables of a different class, which require a deeper soil, and thus by slow degrees, a sufficient thickness is obtained for the growth of the most perfect plants.

This has repeatedly taken place, as is evident from observations on the soil in the neighbourhood of Naples. The same thing that happens to the lava of volcanos, would happen to all the rocky ground in the country, were it not that on account of the rocks being for the most part greatly inclined to the horizon, the greater part of that which moulders in consequence of exposure to air, is washed down by the rain, or blown away by the wind. Thus the soil



does not accumulate, and is only fit for the support of lichens and mosses.

The soil derives its character principally from the stratum of earth immediately below it; though it is not entirely composed of it. Sand and gravel, the calcareous earths and clays, with sometimes magnesia, form the principal, if not the whole of the earthy part of every soil. In some cases, these are mixed with much animal and vegetable matter, and frequently the soil consists of the roots, stalks, and leaves of plants mixed with a quantity of water. Hence soils have been divided into the sandy or gravelly soil, into the chalky soil, into clayey soils, and into the loamy, by which last is meant a quantity of animal and vegetable matter mixed with sand or clay. To these may be added the mossy soil, consisting principally of the roots, stalks, and leaves of plants in a state of imperfect preservation mixed with water.

To each of these divisions a number of soils differing in some respects from one another belongs; though the article from which they receive their classical name be the prevailing ingredient in the composition of all. These soils differ in the nature of the plants belonging to each one, which grow with greater vigour, and arrive to greater perfection in it, and which degenerate or die if removed into a different one. It is in dry gravelly soils that [fir and other resinous trees arrive at their greatest perfection, and grow most vigorously. They are unhealthy and do not attain such size in a clayey soil.

The poplar and the willow tribe thrive best in soft, wet soils, and are unhealthy in dry gravel. Even stagnant, spring, river, and sea waters have each of them plants peculiar to them, which they maintain in the greatest health and vigour, and which die or become unhealthy by removal into a different soil.

Every soil, therefore, may in a certain sense be said to be fertile because it produces certain plants in greater quantity, and to a greater perfection than any other soil would do. But this is not what is commonly meant by the term fertility of soil. There are certain plants necessary for the support and luxury of men, which are scantily furnished by nature, but may be obtained in great quantity by art. The soils which produce these plants plentifully, and in the most perfect state, are called fertile; and the soil is said to be barren if it will not produce them at all, or sparingly and imperfectly. These plants grow in very few soils; they require great attention, and no soil, however fertile it may be at first, is capable of continuing so for any length of time. The crop becomes every year worse, and at last, fails altogether if the soil is left to itself, and the same plants kept constantly growing in it.

The fertility of the soil depends, partly on the proper distribution of those things absolutely necessary to vegetation, partly on an attention to circumstances, which, though not absolutely necessary to vegetation, greatly increase its vigour. To the first,

belong the effects of earth, air, water, and light upon vegetables. Water is absolutely necessary to fertility, for without it no vegetable can grow. In the same manner, air is one of those things necessary to fertility, for when air is excluded, no vegetable will grow. A certain degree of heat is likewise necessary to vegetation; without it, no vegetable could grow, because, otherwise, neither water nor the other substances could be absorbed by the plant. Light is necessary to the perfect growth of plants. When light is excluded, the vegetable grows; but never arrives to perfection, and never acquires colour, taste, smell, nor medicinal efficacy. The fertility of soil seems to depend on its transmitting water and air in proper quantities to vegetables. When the soil furnishes these in small quantities, or not at all, or only one of them, it is barren. The fertility of the soil is increased by every contrivance by which water and air can be furnished in a proper manner. Of this kind are those operations in agriculture which consist in the mixing substances, called manures, with the soil, by which it yields greater crops than it would otherwise do, principally by furnishing more air to plants, but partly by altering the nature of the soil with regard to its powers of furnishing water. We likewise increase the fertility of the soil by sheltering the plants, which we are cultivating, from the inclemency of the weather, by hindering them from being shaken or broke, or torn up by the winds, or chilled by the

frost, or injured by water stagnating upon them, and by preventing the soil in which they grow from being washed away by torrents. All these operations tend either to make the soil furnish water and air in proper quantity to vegetables, or to make the vegetable receive the whole of these that it is capable of, without being disturbed by external accidents.

The fertility of the soil depends on the proper distribution of water, air, heat, and light. The first of these circumstances is what we have most in our power, and to it most of the operations in agriculture that may be called mechanical are referred. The distribution of air, so far as depends on the chemical nature of the air, can only be affected by the use of manures, and the heat and light we are obliged to leave almost entirely to nature, as we cannot for any length of time preserve a quantity of ground of a different temperature from that natural to the climate it belongs to, or make more light fall on it than the heavenly bodies already direct to it. The fertility of the soil depends more on the proper distribution of water, than upon any thing else. Excepting in transmitting water to vegetables, the earthy part of the soil has no share in its fertility, as is evident from the experiment of Van Helmont, by which it appears, that no part of the soil enters into the composition of the vegetable. He took a pot of earth, and dried it in an oven, and weighed it accurately; he then planted in it the seed of a willow. The plant grew with vigour, and after a

certain time, had acquired a considerable size; he cut it down, and found it weighed 50 lb. He then carefully separated the roots from the earth, and dried the earth in the pot, and found it weighed the same as at first. This experiment shews clearly, that none of the soil enters into the composition of the plant which grows in it. It has generally been concluded from this experiment, that water is the only nourishment of vegetables. But Dr. Hales has shewn, that every plant absorbs a great quantity of air, which is as necessary to it as water, and chemical experiments demonstrate that certain dry plants contain one-fourth of their weight of air, and in some cases more.

All vegetables require water in considerable quantity. The disposition of any soil to nourish in perfection certain plants, depends almost entirely on the quantity of moisture which it is disposed to transmit to the vegetable which grows in it. But though the proper transmission of water be of much importance, the nature of the earthy part of the soil affects the fertility from other circumstances. Of this kind, are its being easily or difficultly penetrated by the tender roots of plants, and its having such a degree of cohesion amongst its parts, that the roots of a vegetable may not be pulled out of the ground, or any way injured by external accidents. If the soil is impervious to the roots of plants, or if it is pervaded by them with difficulty, whatever quantity of moisture or other substance necessary for the growth



of plants the soil may contain, but little of it can be transmitted to the vegetable on account of the small surface which the roots of the plant expose to the soil from which the nourishment is drawn. In these respects, different soils differ extremely from one another. In sandy and gravelly soils, as they consist of parts entirely incapable of being affected by water, the moisture is readily imbibed, and as readily passes through them, or they part with water as readily as they receive it; the roots of plants insinuating themselves with ease between the particles of which the soil is composed.

A clayey soil consists of parts easily affected by water, in consequence of which, upon being moistened, they swell to a considerable size. Water, however, does not penetrate them as readily as it does sand or gravel. A quantity of water thrown on dry clay, moistens only its surface. The greatest part runs off, while the internal parts remain nearly in the same state as before. When clay is thoroughly wet, it parts with its water very slowly; it contracts in proportion, becomes harder and firmer, while many cracks form themselves in it. The roots of plants penetrate it with difficulty. The effects of water on these soils are opposite. Hence a quantity of clay thrown on a sandy soil, by hindering the water from passing so readily through it, enables it to nourish plants more perfectly than it would have done without this mixture. And, in the same manner, when a quantity of sand is added to a clay



soil, its parts are divided ; its tenacity is diminished ; the roots of plants penetrate it with great ease, and it both receives and parts with water more readily than before.

Most soils consist principally of these earths, either separate or combined, and those soils are naturally most fertile which consist of a mixture of both, and where there is a little more sand than clay. By breaking the soil into small parts by mechanical means, the interstices of the particles become smaller and more numerous, and better calculated to retain and transmit moisture, while the roots of the vegetable penetrate the soil with greater ease, and spread themselves by this means to a greater distance so as to draw more nourishment. Ploughing or digging the earth has universally been allowed to promote its fertility, and that every advantage in vegetation depends on the proper transmission of water is evident from the experiments of Mr. Bonnet, of Geneva, who reared many plants by putting the seeds in cotton, or sponge, or moss, and watering them from time to time. The plants would not grow to equal perfection in water, because they would by this means have absorbed more water than is necessary for their most perfect growth, while the water, when the roots of plants are immersed in it, is apt to spoil them. Every vegetable, from Hales's experiments, requires a certain quantity of water for supporting it in the most perfect state. If more or less than this is allowed, the vegetable decays, and

equally so by too great a quantity as by too little. In Mr. Bonnet's experiments, the vegetables did not merely send forth stalks and leaves, but continued to grow with vigour, so as to produce fruits and seeds in perfection, the great points to which all vegetation tends, and on account of which solely, the stalk, leaves, and flowers are produced. He raised vines in this manner, which produced grapes not inferior in flavour to those produced in the richest soils in the neighbourhood, and the lemons and oranges growing in sponge moistened with water, were equal in every respect to those produced in the best soil, and with equal attention. Great care no doubt, was taken in distributing water to the vegetable in these experiments. Without this, it is impossible he could have raised such perfect plants. He probably imitated nature in this respect. He would allow them plenty of water at the beginning; this quantity he would gradually diminish, as Hale's experiments shew that the absorption of moisture by plants, diminishes in proportion as the fruit approaches to maturity, and ceases almost entirely when that is completed.

Vegetables absorb likewise a considerable quantity of air. This they do partly by their roots, and partly by their leaves. The air absorbed, is either common air, or air of a different nature, such as has contributed to the inflammation of combustible bodies and respiration of animals, or which is separated from animal and vegetable bodies during their pu-

refaction. The leaves of plants seem to absorb common air in considerable quantity, but they absorb likewise the other kind, or fixed air, or rather separate from it that part which it contains, upon which its difference from common air depends. The soil in which vegetables grow contains a quantity of atmospheric air between its particles, but this probably is not solely that which the roots of plants absorb; for, if this were the case, every soil which transmits water equally, should be equally fertile; but this is not agreeable to experiment. Pure earth, and earth taken up from a considerable depth, that has never nourished a vegetable, is not fertile. Few plants will grow in it, and those of the most imperfect kind. Yet, there is as much common air between the particles of this soil, as in soils that are more fertile. By exposing these soils to the air, they in time, acquire a vegetative power, partly by attracting air, and partly by catching that vegetable and animal matter which is floating in the atmosphere.

From every soil which is fertile, a quantity of air of another kind, the fixed air, can be separated, which is furnished by the vegetable and animal matter contained in it, and is produced by the gradual putrefaction of that matter. It is from wanting this matter, that pure earth is unfit for the growth of plants, and, that soil brought from a great depth, is so long in becoming fertile. This vegetable matter

contained in every soil, affects its fertility in two ways, partly by modifying the power of transmitting water to the vegetable, and partly by furnishing air to it. It disposes sand to retain more moisture for a longer time in it, and to part with it more gradually. By dividing the particles of clay, it renders it more disposed to receive and part with its moisture, and more easily penetrated by the roots of plants; when left to itself, it gradually rots, and furnishes a quantity of air sufficient to support a certain number of plants. If, by any means which hasten its putrefaction, a quantity of air is produced from it, a greater number of plants will grow in it; a greater crop will be produced, but in time, the whole vegetable matter being destroyed, the fertility ceases, and the soil is reduced to the state it was in before any plant had grown in it. We can restore its fertility, by mixing it with a quantity of putrid animal or vegetable matter.

Vegetables differ greatly from one another in the quantity of vegetable matter which they leave in the soil, after the part above ground has been separated. Some of them leave more vegetable matter than was employed in furnishing them with air: but this in time produces a change in the soil, with regard to its power of transmitting water, which renders it unfit for the perfect support of the vegetable, in consequence of which, the soil ceases to be fertile for that vegetable. Others leave less vegetable mat-

ter than that employed in furnishing them with air, on account of which, the same soil ceases in a short time to be fertile. By raising these plants alternately in the same soil, their effects, in a great measure, correct one another, and a greater crop can, for a number of years be thus obtained, than by raising only one of them. It is principally in this way that the fertility of the soil can be preserved for a long time, by raising the filiquose or leguminous plants, alternately with the annual culmiferous plants. Some plants, as trees, produce little alteration in the quantity of air contained in the soil, and can, therefore, grow in the same soil a long time. These require little air for their support, at least, absorb little by their roots, and are, therefore, raised most easily by water, as is evident from Van Helmont's and Bonnet's experiments, where little air could be attracted by the roots, the leaves attracting the greatest part necessary for vegetation.

Vegetables may be injured by a too great quantity of air transmitted to them, as well as by too much water. The use of putrid animal or vegetable matter mixed with the soil is therefore limited. When used, however, in a moderate quantity, they afford the most certain method, at all times, of promoting the fertility of the soil.

Frequently, the vegetable matter contained in the soil has some small remains of vegetable life. While it retains this, it does not rot, or furnish air. By



disturbing the ground frequently, so as to hinder any vegetable from growing in it, we destroy these remains of life. Many substances, known by the name of manures, owe their efficacy to their power of extinguishing this vegetable life, and disposing the matter to rot, and produce air. The principal substances of this class are calcareous earths, or marls, the ashes of burnt plants, and common salt. The first of these genera contains chalk, shells, and lime, or lime-stone. Calcareous earth may increase the fertility of the soil in three ways. First, by altering the soil with regard to its power of transmitting water: secondly, by decomposing salts which the soil contains, and which are noxious to vegetation; and lastly, by promoting the production of air, from the vegetable and animal matter in the soil. The first of these it will produce most remarkably in sandy soils, by rendering them more compact, and more disposed to retain water. It produces its effect, in a certain degree, upon clay, by dividing its particles. It is seldom, however, that it is mixed in such a quantity with the soil, as sensibly to produce either.

The second effect it does not produce at all in sandy soils, for they seldom contain any thing saline. But it produces it remarkably in clays, as many common clays contain a considerable proportion of vitriolic salt of iron. This is very noxious to plants; they are always sickly, and of a rusty hue when they



grow in soils containing this. The calcareous earth decomposes this salt, and separates the acid, converting it into a salt less noxious to vegetables, and less soluble in water.

The third effect the calcareous earths produce in a very remarkable degree, as is evident from Pringle's experiments, where vegetable and animal substances, mixed with calcareous earths, always rotted sooner, and furnished a greater quantity of air in the same time, than they did, when exposed, in similar circumstances, without that addition. Calcareous earths are used as manures, either in the form of lime, or in their crude state. When applied to the soil in the form of lime, they soon attract from the air that substance which they had lost by the burning, and operate, therefore, on the soil, in the same manner as if they had been in their crude state. The only use of burning the calcareous earth into a lime for manure is, to reduce it into a fine powder with greater ease than could have been effected by mechanical means. Common salt, though it be generally employed to preserve animal substances from corruption, has been found to promote putrefactions, when used in small quantities. The effects which it produces on the soil are the same with the effect of the calcareous earth. The more vegetable and animal matter is contained in any soil, the greater effect will be produced on it by the calcareous earth and common salt. If the soil contain no vegetable or animal matter, they produce no sensible effect.

If they promote the corruption of the vegetable and animal matter in the soil, they leave less of them in it, in a given time, than it would have contained, if the soil had been left to itself. They promote the fertility of one year, by diminishing the materials necessary for that of the next, and, if used too frequently, will, in time, destroy the fertility altogether, as has happened on many occasions. When the soil is thus rendered barren, it cannot be made fertile in any other way than by mixing with the soil a quantity of vegetable and animal matter, unless by leaving it to itself, when it will, after a long time, supply itself with vegetable matter, like the soil which has been brought from a great depth, in which no plant had ever grown:

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#### NOTE BY THE EDITOR.

It is now believed by many, that plants absorb by their roots some liquid vegetable matter held in solution by water, in a manner not very clearly understood, but of which the existence is very probable. It is indeed obvious to any person, who looks at the water draining from a heap of manure, that carbonaceous matter is held in solution. There have been also experiments not wholly agreeing in result with Van Helmont's and Bonnet's, by Hassenfratz and others, from which it has been concluded, that seeds and bulbs furnish the carbon of which plants are formed when they grow in water, and that in Van Helmont's

celebrated experiment, carbonaceous matter had access through the sides of the earthen pot. By these means some doubt has been thrown on the origin of the hard substance of plants, but the subject is by no means perfectly elucidated. The experiments of Bonnet are too striking to be given up without further investigation.

An allusion is made in this essay to the power of plants to decompose fixed air, and to restore it to the atmosphere in its original state. It is here proper to observe, that Dr. Irvine always held himself to have been the first who suggested the probability of this power of vegetation to renew the purity of common air, by reducing carbonic acid to its principle.

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## ESSAY VI:

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### ON THE SEEDS OF PLANTS.

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**P**LANTS are composed of a great number of vessels adjusted to each other with the greatest exactness. By these they are fitted for receiving and transmitting different fluids: They are furnished with organs by which the nourishment which they receive from earth, from water, air, and light, is changed into new compounds, so as to form juices peculiar to each plant. No art is capable of separating from the substances by which vegetables are nourished, products in any respect similar to those prepared in the bodies of vegetables themselves. Different vegetables nourished by the same soil, water, air, and light still preserve their specific differences, not only in their external form, but in their intrinsic qualities.

Many attempts have been made to discover in what manner vegetables were capable of separating from the substances by which they are nourished, juices peculiar to them. The opinion which has been most commonly entertained on this subject is, that the soil contained all the parts found in vegetables, and that each plant had a power of separating from it that matter which best suited its particular nature. This notion has not, however, been supported by experiment; nothing has been found in the soil excepting water and air. The various juices found in different plants are evidently derived from another source, as I have formerly endeavoured to shew.

After the structure of the human body had been for some time carefully investigated, it was found that many of its functions could be explained immediately from its structure. The operations in vegetation, it was imagined, might likewise be better understood by a similar inquiry into the structure of plants. Many persons accordingly examined their structure and many curious discoveries have been made in the vegetable œconomy. Much, however, still remains to be done on this subject, before the various operations in vegetation can be fully understood. The discoveries which have been made concerning the structure of plants are very numerous; it is impossible to mention them all in one essay. A few observations on the seeds of plants will form my present subject.



The propagation of the species is the great end of all vegetation. Every part of which vegetables are composed tends directly to this point. It is to effect this, or to be conducive towards it, that plants have roots, stalks, leaves, and flowers. Of these the flower is the part most immediately connected with the propagation of the species. The formation of the seed depends entirely on it. Whenever flowers appear on plants, we are certain that the seed is immediately to be formed ; but the roots and leaves of plants are not so immediately connected with the formation of the seed, though they are likewise useful in this respect. A vegetable may produce stalks and leaves for a long time without producing any seed.

There are great differences among vegetables with regard to the time they must grow before the flower and seed make their appearance. In some plants the seed is produced always in the first year of the growth ; in some the flower does not appear till the second year ; and in others the plant must produce leaves for a great number of years, before it acquires sufficient strength to ripen its flower, and send forth its seed. When plants have produced their seeds in the greatest perfection, the powers of vegetation immediately become languid, and are no longer exerted with their wonted vigour. In some plants the moment the seed is completely formed, the vegetation entirely ceases, and never can again be renewed, but the plant totally withers and dies. In

others, after the seed is formed, the plant languishes for some time, and then after resting a while, recovers its power of vegetation, so as to produce seed a second or a third time, and in some for many hundred times.

The period during which the vegetation goes on in this languid manner in those vegetables in which it is capable of being renewed, is different in different plants; it even differs in the same plant according to the warmth of the climate in which it grows. In some cases, even in the same plant, one part of it may be covered with flowers and growing with vigour, while another part of it is in that languid state, which constantly follows the ripening of the seed. This takes place chiefly in the stronger plants, which have a woody stem, and grow to a great size. The trees which grow in the torrid zone have frequently flowers on one part, while the rest is loaded with ripe fruit. Sometimes trees are found in this situation even in this climate, but it never happens excepting in uncommonly warm seasons.

In those plants which grow vigorously, and produce seed and languish alternately, the flower and seed, which appear in any one season, are not the produce of that season. The flowers which are to appear the next season are all completely formed already; they begin to be formed in August, and are at the close of the year, when the season is so cold that all vegetation seems to be at an end, increasing in size, and gradually unfolding their parts.

Even the leaves of plants, which are to make their appearance with the returning spring, are then perfectly formed, and require only the assistance of a little warmth to make them expand, and this happens not only in the stronger and more rigid plants, such as trees and shrubs, which endure the cold of winter without injury, but to plants of a more tender kind, which die to the ground in winter. In these plants the leaves and flowers of the succeeding year are formed at the end of the season on their roots, which being covered with a quantity of earth are thereby secured from the winter's cold.

Those plants which produce flowers and seeds but once during the period of their growth, are solely employed in producing leaves and flowers, and ripening their seeds. Whereas the plants which produce flowers and seeds a great number of times with intervals, during which their vigour diminishes, have a much more complicated task to perform. In any one year they have flowers to expand and seeds to ripen, which they had formed the last year, and the flowers to form which they are to produce with seed to be ripened the next year. As they have more to perform, they require more strength to be able to do it; and it is not improbable that it is owing to this that most plants which are capable of producing seeds a great number of times, are several years after they are raised from the seed incapable of producing either flowers or seeds, but increase in size or strength, after which they begin to propagate the species. In

this respect these plants resemble in some degree the more perfect animals, in which it is some time after their formation before they arrive at the age of puberty, so as to be capable of propagating their species. Whereas the plants, which can produce seed but once, and which do it the year that they are raised from the seed, have some resemblance to the more imperfect animals. The plants of this class are commonly called annuals; those of the other are called perennial plants.

There is perhaps some difference in the structure of the seeds of the plants in these two classes. It seems not to be improbable that the seed of an annual plant is more perfect than the seed of a perennial one. The whole of vegetation in the annual plant is directed to the formation of the seed. In the perennial one, the vegetation is directed partly to this point and partly to the formation of the leaves and flowers of the succeeding year.

As the perennial plant has more to perform, it may, notwithstanding its greater strength, form the seed, which is but a part, less perfectly than the annual plant can form its seed, which is the whole intention of its growth. That this is really the case, seems to be evident from the resemblance there is between the buds of perennial plants and the seeds of annual ones with regard to vegetation. The flowers and seeds of perennial plants which are produced for the service of the next year, are most curiously fitted up, and appear in the form of little

conical substances called buds or germs. If we take the buds and put them in ground, they frequently take root, grow, and produce a plant exactly resembling, in appearance, the parent plant; but which differs from it in one respect, that it produces and ripens its seed much sooner than a plant of the same kind raised from the seed. Very frequently a plant raised in this manner will produce seed, and propagate the species the first year of its growth; while the same plant raised from the seed would have required five or six year's growth before it was capable of propagating the species. That this is the case, the constant practice of all gardeners is a proof, who know well that when they want to furnish a garden with perennial plants, there is not a more certain method than to raise the plants from slips. This practice succeeds best in those plants whose trunk dies down to the ground in winter, and where the buds adhere to the roots of the plant.

It may indeed be said, that in this case, the plant is capable of producing seed sooner, not because the bud contains the young plant more perfectly formed than the seed did, but because it has more strength, and because it has one part completely formed, viz. the root. By this means it is better calculated for absorbing from the soil the nourishment proper for it, and can digest more of it than a feeble root just formed from the seed is capable of doing. In answer to this objection, it may be observed, that the experiment succeeds equally well, and the plants

produce flowers and seeds as soon, when the simple bud is put into the earth, as when a quantity of root adheres to it. In many plants, however, this cannot be done; the bud and the root are so connected, that the one cannot be separated without a part of the other adhering to it. But it can be done very perfectly in many plants, and particularly in all the bulbous plants. In these plants there is none of the root of the parent plant that adheres to the bulb, from which a new plant is to be produced. In the root of a tulip as it is commonly called, but which is really a bulb or germ, there is none of the root of the old plant adhering to it; it will however grow, and flower, and produce seed the year in which it is planted, while the seed of a tulip, upon being sown, will produce a new plant, that will not bear seed sooner than the sixth year after the seed is sown.

Though this experiment succeeds best with the plants which carry buds on their roots, it is not solely confined to them. In many trees the same thing takes place, though the buds adhere to the trunk and branches. In fruit trees it is well known, that fruit resembling that of the parent plant cannot be produced by merely sowing the seed of the tree.

Thus the tree may be propagated by making a bud of it grow upon the root of another plant, by inoculating or grafting on it. It frequently happens in grafting particularly, that the bud produces flowers and fruit the very first year of its growth. This however, those who are employed in culti-



vating fruit trees take pains to discourage, as the languid vegetation which would succeed the ripening of the fruit, would be hurtful to the bud in this artificial manner of making it grow. That this hasty formation of flowers and seed from a bud by grafting, in one year, while it would have required many years before this could have been effected in a plant raised from the seed, is not owing to the strength or vigour of the roots of the stock upon which the bud is made to grow, is evident from our being frequently able to accomplish the same end without the assistance of the root and trunk of another plant, by making the bud strike root, and grow immediately without any assistance. This, however, cannot in fruit trees be effected, that I know of, by cutting off the bud and planting it in the earth. But what cannot be done in this manner directly, can frequently be done by bending a branch of the parent tree with buds on it, till it touches the ground, and then covering the branch with earth. The bud will in this situation unfold itself, produce a plant exactly similar to the one it was produced from, in the same manner as if it had been grafted, and will produce flowers and fruit in a much shorter time, than a plant raised from a seed of the same tree could have done; and this happens though the connection betwixt the bud and the original plant be cut off the moment the bud is expanded.

The buds of the perennial plants, and the seeds of annual plants, agree much better in the time re-

quisite for the propagation of the species than the seeds do : and there seems to be some reason to conclude that the seed of a perennial plant is less perfect, is simpler, and fitted for producing a less perfect plant than the seed of an annual one. That this really is the case seems evident, by attending to the structure of the seeds more minutely, where it will be often found, that there is a much more striking resemblance betwixt the seeds of the annual plants and buds of the perennial, than there is betwixt the seed of the two kinds of plants. The seeds of all plants contain the rudiments of the plants to be produced. Sometimes the seed consists entirely of this part, which is commonly known by the name of corculum or germ of the plant. In other plants, besides the germ of the future plant, there is another part closely connected with it, known by the name of the cotyledons or lobes of the seed. Whether the seed of the plant consists only of the germ, or of the germ and lobes connected with it, in both cases they are concluded in a number of coats, which, so far as has been discovered, are inorganic and incapable of corruption. The germ is properly the seed of the plant, the other parts are useful during the infant state of the plant, but form no part of it. It is only in the seeds of one class of plants, the mosses, that we meet with a naked germ. In the seeds of all other plants, besides the germ, there are cotyledons or lobes united with the germ. In some plants, there is only one

lobe ; in most plants there are two ; in a few there are four, six, or ten lobes.

When we examine the germ in the seed of plants, we see that it is composed of two parts ; the one external, which is to become the root of the plant, and called radicula, or radicle ; the other more internal, and buried in the lobes of the seed, or lodged between the lobes, when there are more than one, the plumula or plume, which, after extending, becomes the stalk or leaves of the plant. The radicle is always of a conical figure without any divisions. The plume has no regular figure, and is divided into a great number of small parts. When the seed besides the germ consists of lobes, the radicle sends off a branch to each of them, which is ramified through them, and forms what is called the seminal root of the plant. When the plume is viewed with the microscope it appears to be composed of a number of parts shaped like the leaves of the future parent plant, and requiring only to be a little larger to resemble them exactly. An author who has written lately on the structure of the seeds of plants, but whose name I cannot at present recollect, mentions some curious observations on the seeds of plants ; and from his observations, as far as I remember, it appeared that in some plants the plume, viewed with the microscope, contained not only the parts to become the stalk and leaf of the plant, but that frequently the flower of

the plant could be perceived in it with all its petals and stamina. I remember further that in the instances where this happens the seeds were all the seeds of annual plants, and that nothing of this kind has been observed in the seeds of perennial plants.

This, I think, in some cases at least, shews that the reason why some plants propagate their species in a short time, is, that in the seeds all the parts for this purpose are already completely formed, and require only to be increased in size, while in the seeds of those plants which require a long time before they flower and produce seed, the plumule contains only such parts as are to become leaves, the production of the flower is left to the young plant itself, which in time it produces in folds in the form of buds at the end of the summer preceding the spring in which they are to make their appearance in open day, and to propagate the species.

Though, however, there seems, from the foregoing observations, to be some reason to believe that there is a difference betwixt the seeds of annual and perennial plants in the formation of the germ, in consequence of which one propagates the species sooner than the other, we cannot conclude that this is the case in all. In many plants that are annual, the presence of the young flower in the seed has not been discovered, partly perhaps from its smallness,

and partly from its being enveloped or concealed by the other parts of the seed. There may likewise be considerable differences in the structure of the plume of perennial plants, especially as we find that they differ remarkably in the time which they must grow before they produce flowers and seeds ; those that require the shortest time may have their seeds constructed like the annual ones ; but no observations of this kind have been made that I know of ; and perhaps those plants, whose seeds most nearly resemble the seeds of certain annuals, will be found in the circumstances of the vegetation to approach nearer to the annual plants than those perennials which differ more in that respect. They probably will be found not only to propagate the species sooner, but for a less number of years than the rest of the plants of the same class do.

Another difference in the seeds of plants is that some of them are capable of drawing their nourishment from the soil, and preparing it for all the purposes of vegetation the moment they are buried in the earth ; while in other plants the seed when sown, is some time before it is capable of doing this. In those plants which consist of a naked germ, the moment they are sown the radicle becomes a root, and draws that part immediately from the soil which is for its support, without requiring any assistance, or any other nourishment than what the soil immediately furnishes. The seeds of all the mosses are of this kind. They are ranked among the most im-

perfect vegetables, and require less nourishment, and that of a weaker kind, and their young is more hardy than the young plants of the more perfect kinds of vegetables. In the seeds of the more perfect plants it is some time before the radicle is capable of collecting that nourishment from the soil which is requisite for the support of the plant. The nourishment too which the soil contains, or which is contained in the other bodies which surround it, is of too coarse a kind for the support of the tender plant; or it cannot prepare it properly. In these plants, besides the germ there are the cotyledons or lobes of the seed, which assist the radicle and plume in the first period of their growth, partly by absorbing from the soil that nourishment which is best fitted for the plant, and transmitting it through the seminal root to the radicle and plume; but more especially by a quantity of food prepared by the parent plant, of which, besides the seminal roots, the lobes of the seed are almost intirely composed. This substance in the lobes is sometimes intirely farinaceous, in others partly farinaceous and partly of an oily nature, and in others farina blended with other substances. It is intirely farinaceous in the common kinds of grain, as wheat, barley, and oats. In these plants there is only one lobe, and the plume and radicle are lodged in a hole at one end of it. In the almond, the poppy, and many other plants, there are two lobes, which consist partly of farina and partly of oil.



The matter which the lobes of the seed contain is deposited there, to serve for the nourishment and growth of the germ of the plant in the first days of its vegetation. It is a nourishment ready prepared, and already digested by the parent plant for the support of the young plant which it is to produce. It is the aliment of their earliest age, while they are yet too weak to extract from the earth and other elements the materials which they are to change by their organic action into their own substance or juice peculiar to them. In the same manner as the yolk of an egg serves for the nourishment of the chick, while it remains in the shell; and as the milk of animals furnishes to their young a nourishment already half assimilated, the digestion of which is easy, and proportionable to the weakness of the digestive organs. When the seeds of these plants are sown which have nourishment already prepared for them in the lobes by the parent plant, the moisture of the earth pervades the coats with which all seeds are covered, its impurities are by this means separated, and it is absorbed by the lobes more gradually; the matter in the lobes swells with the moisture, a part of the nourishment in them passes into the seminal root, and is transmitted to the radicle and plume of the plant, which begins immediately to increase in size. First the radicle extends itself, and then the plume. During this period the seminal root exerts some assimilating power, and the nourishment in the lobes changes its sensible qualities till it is ex-

hausted in nourishing the young plant. The radicle and plumbe having by this time acquired sufficient vegetative power and strength, can prepare for themselves from the earth and other elements the nourishment and juices fit for their support and growth. The lobes having thus for some time served as a root to the young plant, are no longer fit for this purpose. They fall off and decay, or begin to increase in size, and having for some time served as root, till the radicle became sufficiently strong, they now become leaves, and assist the plumbe till its parts are sufficiently unfolded, when they fall off and wither, having served all the purposes for which they were intended.

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## ESSAY VII.

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### *ON THE ROOTS OF PLANTS.*

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**I**N a former essay it was observed, that the seeds of all plants consisted principally of the rudiments of the root and stem of the future plant : that the seeds of one class of plants consisted solely of those parts : they are capable of drawing directly from the soil the part necessary for their support. The plants thus constructed are of the less perfect kind ; they never grow to a great size, and their nourishment seems to require but little preparation. They accordingly subsist upon the most barren parts of this earth ; and serve principally to produce in time a sufficient thickness of soil, for the nourishment of the more perfect vegetables. The seeds of the most perfect plants contain, besides the rudiment of the root and stem, a part containing nourishment for the young plant, intended to support it, till its own root be capable of drawing from the earth nourishment proper for it. During this time the young

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Send off lateral branches, the nourishment contained in the seed, and which was prepared by the parent plant, is nearly exhausted. That part, therefore, falls off, or by a little alteration becomes a sort of leaf, and supplies the place of these parts till at last it falls off altogether. When the nourishment of this secondary part of the seed is exhausted, the root begins to prepare nourishment for the plant. At first the quantity necessary is but small, as the size of the plant above ground is trifling. As this increases, the root extends itself to be able to collect and prepare proper nourishment, and to give that stability to the part above ground, and even that direction to it which is most proper for the particular plant. Hence the distance to which the root extends, and the strength which its parts acquire, necessarily depend on the height and size of the part above ground.

The parts in all plants that imbibe moisture are probably the small and almost invisible portions of the roots. The thick fibres are probably intended to give them support, or perhaps may be otherwise necessary in preparing those fluids imbibed by the capillary fibres of the root. When a plant is dug up with great care, none of the large fibres are destroyed, yet the growth of the plant is always very much retarded by it, and very frequently altogether destroyed.

The organization by which the roots of plants draw ~~nourishment~~ from the soil, is the same in all plants.

The discoveries made in the structure of the roots of plants have not hitherto enabled us to explain their growth, any more than the anatomy of animals has enabled us to explain their functions; both are still out of our reach. But it is to have proceeded one step in the discovery to know the organs by which this is done. The manner in which they operate may, perhaps, be some other time explained to us. We know that the roots of plants consist of a great number of vessels, most curiously interwoven, that beauty and elegance is given to these vessels by means of a soft spongy substance, the pith, which is contained in the centre of the root, and readily accompanies every production from it: and that the whole is covered by a fine membrane like the scarf-skin of animals, which defends the rest, and serves as a strainer to the juices absorbed by the vascular part of the root. This structure, however, has not unfolded to us, why a vegetable has life without voluntary motion. But these organs are necessary; if any of them are taken away or injured, the growth of the plant is injured or destroyed.

The substance absorbed by the roots is the same in all plants; the quantity of nourishment necessary for their support is different in different plants. The first is evident from chemical experiments which shew that they are composed of the same principles, and Dr. Hales and Mr. Bonnet have proved that plants absorb from the soil only water and air, at least that water and air will support plants equally.



well with the soil in which they naturally grow. That the quantity of nourishment or rather the readiness with which it is administered is different in different plants is evident from the different situations in which they naturally grow. Every part of this earth has plants adapted to it. The rock of a volcano, the richest soil, the field, the marsh, the moss, the pond, the river, the sea, have plants peculiar to them. In these different situations water is furnished more or less plentifully, and a difference in the shape of the part for absorbing that moisture becomes necessary. The size a plant grows to, the position it requires to be placed in, the time that it grows, is different in different cases, and an alteration in the root, the principal part in plants, becomes on these accounts necessary.

The age of a plant depends intirely on its root : some live one year, some two years ; others many years. In those plants that live only one year it is rarely that they acquire any remarkable size, they need, therefore, but little support to keep them in a proper position, and a small quantity of nourishment. The root is in them small, and there is no reason for its spreading to any great distance, or becoming strong and woody. The roots of the annual plants consist generally of a few fine fibres, which sink only a little way into the ground. The same observation is applicable to the biennial plants ; they are never large, and require but little support or nourishment ; and it is principally by alterations in the

figure of their roots, that they are fitted for different soils.

It is in these plants, that last for many years, that the greatest diversity of roots is observable. As the number of years they are disposed to last is very different, they differ greatly in size, and their roots vary prodigiously from each other, in the direction in which they sink into the earth. When the roots of plants are said to differ from each other, it ought to be understood to be meant to extend only to those parts in the roots of plants, that are employed in nourishing the plant. The parts that are occasionally attached to roots, such as bulbs, or gems, as they have no share in nourishing the plant, and as they are destined solely for the nourishing of the leaves and fruit of other years, shall not at present be considered.

The most remarkable circumstances, in which the roots of plants differ, are size, figure, and direction, the last of which is the cause of the greatest differences among them. In some cases, the root consists of a great number of small fibres, which spread near the surface of the ground, and form an angle with it from 20 to 30 degrees. In these we seldom find, that they sink above a few inches into the ground, and never below the vegetable mould, and in the largest trees not above a foot or two. Of this kind are the fibrous and the branching roots of plants, which two do not differ from each other excepting in size. The fibrous root is slender

and weak, sufficient to support the smaller delicate plants. The branching root is destined to nourish the larger and more robust plants. When any injury is done to these roots, we seldom find that the plant is injured much. The wounded part, as it is always small, soon heals up, and if a part happens to be cut off, new branches spring, and supply its place. Though this should be done two or three times, the plant not only still survives, but grows with vigour. The only thing injurious to the roots of plants of this kind is shaking them, by which their hold of the ground is diminished, and the more tender fibres, by which the nourishment is chiefly absorbed, are destroyed.

This, frequently repeated, checks the growth of the plant, and, in time, stops it altogether. Another circumstance, which would be perhaps injurious to the roots of the fibrous and branching kind, is, the forcing them much out of their natural direction, and obliging them to sink deep into the earth. Naturally, they sink but a small way into the ground, they require, of consequence, a rich soil for their support. If they are, any how, made to sink deeper into the soil than is natural to them, their growth is hurt, partly by the greater pressure of earth upon them, partly by their being farther removed from the soil near the surface, which, from their natural direction seems to contain the most proper food for them. If roots, therefore, of this kind, are not planted too deep, and if the plant is in such a situ-

a great depth, far below what may be called vegetable mould. If the plant be of any considerable size above ground, it sinks into and spreads among the regular strata of earth. The nourishment it draws is not so rich and substantial, as that contained in the soil at the surface, as is evident from their growth being more checked by cutting and dressing, than superficial roots. The spindle root is a single fibre, which lengthens only at its extremity, like all roots, and sinks perpendicularly into the earth. The surface which this root exposes to the soil is not to be reckoned from that of the principal fibre. That is too small in large trees to be sufficient for the support of the plant; but it must be reckoned from the fibres, which, rising from this trunk, at right angles from it, spread in a horizontal direction. According to the depth of the root, whence they spring, they will draw a richer or a poorer nourishment, and these being afterwards mixed, produce a compound fit for the support of the plant. As the greatest part of the roots of this kind are far below the surface, the soil at the surface will be less impoverished, and be fitter for the support of plants, whose roots are of a different shape. In roots of this kind, if the end of the perpendicular fibre is cut off, or injured by an insect, or prevented from following its natural direction by the hardness of the soil, its growth in length will be stopped, and, of consequence, it will not be able to sink deep enough into the earth

to draw the proper nourishment for a full-grown plant: because the root will not grow to such a size as to afford surface for as many roots to spring, as are sufficient for drawing nourishment to a large plant. This, at first, may not perhaps, apparently hinder the growth of the plant; for a while, it may shoot with seeming vigour; but if the plant is of that kind, that naturally grows to a great size, it will in time be observed, and the plant, upon whose root this injury has been committed, will never grow to the same size, or live to the same age which it would otherwise have done.

These observations on the roots of plants may be illustrated and confirmed, from the culture of all perennial plants, but is most evidently confirmed from the growth of trees, which, having a woody stem, grow to a great size, and afford time for observation, which cannot be had in short-lived plants.

Among the trees, the ash, elm, horn-beam, and sycamore, have fibrous, or branching roots. The poplars are the most remarkable trees with creeping roots. The oak, chestnut, walnut, and beech, are the most remarkable trees with spindle-shaped roots. The ash is the most common tree with fibrous or branching roots. They spread to a great distance, and sink into the ground at an angle of 20 degrees, near the surface of the earth, impoverishing the ground to a great distance; so that the corn-fields in the neighbourhood of the ash are less fertile than the same soil at a distance from them. In the farms in the coun-



ties of Gloucester and Cheshire, they will not allow any ash trees to grow. They say, the cattle eating the leaves have the qualities of their milk spoiled: but it is likewise owing to the ash-roots impoverishing the soil, and spoiling the grass. It is a common observation, that the drop of the ash tree is more injurious than that of any tree. This, however, I do not imagine arises solely from the bad quality of the rain which falls from its leaf; for I have steeped ash leaves in water, and used it in watering young trees and other plants, without finding that they were injured by this treatment. In digging up ash trees, the most of the roots are observed to spread near the surface. In consequence of this it thrives in a shallow soil. It bears transplanting well, but is improper for hedge-rows, from its impoverishing the soil in its neighbourhood. The wood is valuable, but it requires rich land to make it grow with vigour. It is of slow growth, and is not ornamental, from its being so late in the season before it gets its leaves.

The sycamore or plane tree has roots like the ash, but they sink into the ground at a greater angle, and go deeper than the ash. It does not injure grass, is equally easy transplanted with it; grows like it, best in rich ground, but requires a little little lighter soil, and as it gets better hold of the earth, it grows in open fields more easily, but thrives still better in groves.

Pretty much of the same nature is the elm, but



its roots are more of the creeping kind, sink deeper into the soil than the ash, yet will grow very well in a clay soil, and is excellent for hedge rows. All cattle, horses, sheep, &c. are fond of its leaves, and it bears transplanting very well. The horn-beam has the same kind of root, likes poor stiff clay, is easily transplanted, but injures the grass. It is a valuable wood, and it is surprising it is not more cultivated. The wood burns like a candle and is very white and tough, and fit for many purposes in husbandry.

All these trees have fibrous and branching roots. The plane sinks the deepest, and the horn-beam the least into the ground. They all thrive in a shallow soil, with clay or tilly bottom, which prevents the root from sinking deep. They require to be sheltered from the winds to hinder their roots from being loosened, and grow best in groves. None of the roots sink into the ground at a greater angle than one of 35 degrees; they all bear transplanting well, almost at any age, and can then only be injured by planting their roots too deep. The same observations apply to the poplars, only as they have creeping roots, they bear transplanting still better, and, provided they have plenty of moisture, thrive well in any soil, but worst in clay: their roots spread near the surface, impoverish land, and will allow nothing else to grow.

The oak, chestnut, walnut, and beech, have spindle-

shaped roots. They require a deep soil. They do not bear transplanting, and we never find them to grow to their full size, but when in natural woods, where their seeds have been sown in places where they are now growing. When raised in a nursery, and transplanted, they thrive for some time, make a tolerable shew as ornamental trees, but are seldom straight, tall, and beautiful. They always retain the marks of this improper management, and are generally crooked and of low stature. All the trees that bear nuts are furnished with the same roots, and are injured by the same treatment. The fir tribe, in which I include the *larix* and cedar, may be called nut-bearing trees; for they are all furnished with a hard shell, which includes a number of seeds. They have roots somewhat singularly shaped. The principal root resembles a spindle. This sinks perpendicularly into the ground, and they require, accordingly, a deep gravelly soil, and do not bear transplanting. The least injury to the extremity of this root checks the growth of the tree. But they have likewise a great number of small roots that spread horizontally, almost even with the surface of the ground. In consequence of this they destroy the grass in their neighbourhood, and though they do not die when their spindle root is inert, they do not grow to a great size, are unhealthy, and are capable of dragging out a miserable existence for a great length of time.

These observations on the roots of trees point out the reason why natural woods excel those that are planted, because in all cases the roots of trees are injured by transplanting; why the injury is greatest to the roots of those trees that sink deep into the ground; why the ash, the elm, the hornbeam, and even the sycamore may be raised in the nursery, in the usual manner; and why no attention is necessary in the raising of poplars, but to plant them in a wet soil. But if we wish to raise those most useful and beautiful of all trees, the oak, the chestnut, the beech, and the walnut, we must choose a deep soil, and plant the seeds where the trees are intended to remain; all the trouble this requires is but keeping them clear of weeds the first year or two. The chestnut and walnut may thus be restored to this country; and those who may dislike this manner of rearing trees, as being slower than by planting large plants from a nursery, may be assured, that any difference of this kind will not be observable in three or four years. This practice might be extended to advantage perhaps in the fir tribe; though in them, planting them two or three years old from the nursery will answer very well. If the chestnut and walnut are raised for the sake of their nuts, the practice recommended is not proper, it is only applicable when we raise them on account of their wood: but this is so obvious that I shall not take up time by insisting on it.

Neither do the observations apply where large shoots

are wanted, where length is of more importance than any thing, as in shoots for hoops, &c. For this purpose a different soil from that mentioned above is necessary, such as contains much moisture, and is easily penetrated. Thus the ash in boggy land makes large shoots <sup>as for hoops</sup>, and is very profitable.

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## ESSAY VIII.

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### *ON FERMENTATION.*

Read A. D. 1785.

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A BODY is said to have been fermented when its sensible qualities are much changed without any remarkable cause of the alteration being perceived. All vegetable and animal substances are capable of having their properties more or less altered in this manner, unless they have been exposed to such a degree of heat as to dissipate their watery moisture either wholly or partially, and reduce them to the state of charcoal, or dryness. They also may be deprived of their disposition to ferment by the addition of certain substances of the saline, bitter, or aromatic kind. This change in the qualities of bodies is often the only mark of preceding fermentation; though sometimes it is attended through its whole course, with a hissing noise, the emission of

numerous air bubbles, and a copious froth or scum upon the surface.

Though all vegetable and animal substances in their recent or fresh state are capable of having their properties changed, by fermentation, yet the change in quality as well as the circumstances which accompany it, vary much in different individuals. We always find that animal and vegetable substances mixed with much water, fall very readily of themselves into this process. Frequently the eye can discover no change in the properties from which the preceding fermentation can be inferred; yet the alteration in consistence, in taste, or smell afford a certain method of discovering fermentation. In many cases, however, we can perceive that something is going on, and from the quantity of elastic matter separating, we are led by experience to expect a change in the other qualities of the body. These changes, whether induced by invisible or sensible fermentation, have given origin to the different stages into which fermentation has been divided. A body, by fermentation, is sometimes changed into a vinous liquor, of the nature of wine or ale; in others it acquires acid properties like vinegar, with or without the preceding vinous state; and in others the body becomes putrid and rotten after having been in the state of wine or vinegar, and often becomes putrid without having ever been in any of these states. Hence fermentation has been divided into three stages, the vinous, the acetous, and the



**putrefactive.** All substances that are inflammable and capable of mixing with water may be made to ferment in some of these three ways.

As all bodies that ferment are inflammable, so we find generally that more or less heat is produced during the process. The more heat that is produced by the fermentation, the more nearly does the body approach in its properties after it to the state it would have been left in after inflammation. To what this is owing is not very easily assigned, but one is almost led to imagine, that air, which is the principal agent in producing heat during the inflammation of a body, is also in part concerned in producing the heat in fermentation, and is likewise the chief cause of the change in the properties of bodies produced by it. Some of the changes produced by fermentation are useful or agreeable. We therefore encourage the process in various ways. When the change is disagreeable or hurtful, we endeavour as much as possible to prevent it. Though these changes have been long known, and though the industry of man has been long employed to promote or retard them, the cause of them is not yet properly ascertained. Many explanations of them have been given by eminent men, and much has been said upon the subject. It is not my intention at present to give a full account of fermentation: it would take up too much time, and would furnish matter for several essays. I shall confine myself at present to the causes of the differences observed in these bo-

dies, which have undergone the vinous fermentation. It may be proper to observe, that great differences in fermentation arise from the various species of matter of which different substances are composed. According as the body is simple or compounded its fermentation will consist of only one stage, or of two or three joined together. Of all the parts of vegetable or animal substances which may be fermented, those that have a sweet taste are capable of being changed in the greatest variety of ways by it. If the sweet matter is immixed with any other, we can make it first into wine, then vinegar, and lastly putrid; and the order cannot be altered or reversed. It cannot be made into vinegar after being putrid, or into wine after being vinegar.

Vegetable substances furnish this sweet matter in greatest abundance; a little sweet matter is also found in the animal kingdom, but only in the milk of animals. The milk of animals in its fermentation resembles that of sweet vegetable matter, but on account of its intimate mixture with various animal substances, it differs in some respects from it. Nature has furnished a vast variety of vegetables that have a sweet taste; and as along with the sweet matter there is a great quantity of water, the process of fermentation is frequently going on spontaneously, and must have been observed at a very early period. Sweet substances are those only that can be reduced into the state of wine. The exhilarating quality

which the sweet matter thus acquires, made it very generally liked wherever it was known, and the vinous fermentation was probably not only the first species of this process performed intentionally, but likewise the first which became the business of a trade or profession.

We do not know when this discovery was first made; but wine seems to have been a very early species of luxury: in many places it is a necessary of life. In those countries where grapes were scarce, other fruits were substituted; and where fruit was not so plentiful, the farinaceous seeds prepared in a particular manner afforded a liquor possessing the intoxicating quality, though not the pleasant taste and flavour of wine. In countries where neither fruits nor grain were common, or where the manner of fermenting them was unknown, substitutes were discovered which possessed the enlivening or stupifying qualities of the juice of the grape. Some of these were prepared from milk by fermentation, and others are aromatic stupifying plants, such as opium, tobacco, the areca, and beetle-nut, and the kava of the South Pacific Ocean. It has indeed been asserted that no liquor like that which Buchanan alleges the ancient Caledonians employed, can be procured from milk. But whey boiled to a proper consistence may be made to fall into the vinous fermentation.\* The same precautions are indeed requisite, with-

\* A liquor of this kind was used not only by the Gauls but also by some of the Tartar Nations.

out which it will become sour, instead of acquiring the properties of a vinous liquor.

Of all the fruits, the vine is that whose sweetness most easily falls into the state of an agreeable vinous liquor by fermentation. But the wine from the juice of the grape has been known from a very remote period, and the ancient Greeks and Romans were good judges of it, and well skilled in its preparation. Yet they do not seem to have understood the change which the fermentation had produced upon the nature of the juice in the fruit. At least we are entitled to conclude this from their total silence upon this subject.

At what period mankind made the discovery that wine owed its chief qualities to the production of a small quantity of fluid, such as is now called spirit of wine, is not more distinctly known. When it was first discovered it would be considered as very curious, that a small quantity of a colourless fluid being separated from wine, it became nauseous and disagreeable; while the fluid separated, though it had little of the smell or flavour, and none of the taste of the wine, possessed its other properties in a very high degree. The discovery seems not to have been made in the time of Pliny, for he mentions nothing about it. The only thing which looks like it in any ancient author, is the discovery ascribed to the elder Cato, of separating water from wine by putting it into a dish made of dry wood. Wine and water mixed poured into a vessel made

of this wood, were soon separated through the pores of the wood, and the wine remained behind. An ancient author, giving an account of this discovery, says, that the wine passed through, and the water remained behind. Mr. Ray says, in his *Historia Plantarum*, that he tried the experiment, and that no separation of the wine from the water, or water from the wine took place, but that both passed through the wood together. A modern traveller, indeed, asserts, that he astonished some of the wandering Arabs with this experiment; and mentions it as a strong mark of their ignorance, in being surprised at an experiment, which every body in Europe knew how to perform.

The Arabians are said to have first discovered the method of separating spirit from wine, but at what period is unknown. It is, however, not improbable that this was first discovered at the time when attempts were made to form gold and silver.

Substances which have undergone the vinous fermentation, furnish a portion of spirit of wine, which, when properly purified, seems to be possessed of the same properties, though the substances which yielded it differ much from each other. The spirit at first obtained possesses some of the properties of the substance from which it was obtained, particularly a little of the original flavour, so that we can easily distinguish by the smell, and other properties of the spirit, the substance which furnished

it. But even when the differences betwixt spirits are stated as high as possible, they never equal the differences between the vinous liquors which furnished them.

The difference between wines and the spirit which they yield by distillation is owing to the manner of managing the fermentation, and allowing it either to proceed slowly or more rapidly, or stopping it altogether after it has proceeded some length. But considerable differences arise from the various substances with which, in different bodies, the sweet matter is blended. It may, perhaps, not be improper to mention some instances of the effects of each of these, in the management of the sweet matter of the grape.

The quantity of sweet matter in the grape varies in the same species, in different years, and in different soils. In no case, does the juice of the grape consist of mere sweet matter and water: it always contains, besides these, some farinaceous and mucilaginous matter, a small quantity of odorous matter, some of the essential salt of the vine, commonly known by the name of tartar, and a little colouring matter. When the season is favourable and the soil good, the rest of the ingredients bear but a small proportion to the saccharine part, and the quantity of water is also very little. But in bad soils, and unfavourable seasons, the juice of the grape is very watery, and the sweet matter is much diminished. The proportion of the other parts is greater and more perceptible



to the taste; the grape does not acquire that luscious sweetness, but in its riper state has a rough and sour taste. When the juice has such different qualities in different seasons and situations, the liquor produced by fermentation is proportionably various. No part of the juice, excepting the sweet part, can be changed into spirit by fermentation. The acid and farinaceous or mucilaginous parts are either advanced already to another stage of fermentation, the acetous, and cannot be made to return back again to the vinous, or are of such a kind as to be incapable of both the vinous and acetous fermentation. Of this kind are the farinaceous and mucilaginous part, which neither can be made sour nor vinous, if perfectly pure. The liquor produced in this case is more acid and ropy than it would have been if the proportion of sweet matter had been greater.

But the quantity of acid and other parts in the wine are not those only which originally existed in the juice of the grape: a part is produced by the fermentation; though the pure sweet matter would all be changed by it into pure spirit, yet, sweet matter being capable of becoming both spirit and vinegar, by fermentation, according to circumstances, a part changes into the one, and a part into the other, if the fermentation is not stopped precisely at the end of the vinous stage. This is not easily effected, even when the grape is in the most favourable state. When it is not so, when it contains acid parts at

first, it is hardly practicable : for the acid part in the grape is very much disposed to bring on the acetous fermentation from the beginning, and should it fail of having this effect, it is still disposed to make the wine run into the acid state immediately after the vinous fermentation is over.

The farinaceous and mucilaginous parts also have a share in producing this effect. In their pure state, they can neither be changed into wine nor vinegar by fermentation, as has been observed a little ago. Notwithstanding this, when mixed with a little sweet matter, they all ferment together, and may either change wholly into wine, or wholly into vinegar, according to the proportion of sweet matter mixed with them. If the quantity of sweet matter be very great, the farinaceous parts are entirely changed into the same wine, with the sweet part. If the quantity of saccharine matter is very small, the whole becomes vinegar, and has little appearance of ever having been in the vinous state. The mucilaginous part is disposed to alter a little in the same manner, though its effects are not so well understood, or so well confirmed by experiment.

This property of farinaceous matter mixed with saccharine matter has been long known ; and it has been also long known, that farinaceous matter mixed with some other substances, is capable of undergoing still more wonderful changes. Thus, a quantity of meal from wheat, barley, or oats, whose greatest part is farinaceous, when mixed with water,

becomes, by fermentation, a little acid, but seems never to acquire any of the properties of wine, but remains in the state of flummery. If the same meal be mixed with a quantity of sugar, or any other sweet matter, it then falls into the vinous state, and the quantity of spirit produced is much greater than the sweet matter alone would have furnished. We know, that the farinaceous matter, when it constituted a part of the seed of a plant, is capable of being changed intirely into sweet matter. Most kinds of grain have little or no sweet matter in them in their natural state. When put into the earth, they gradually acquire a sweet taste, and retain this during the tender state of the young plant. When the plant has acquired sufficient strength to support itself, the sweet taste is no longer perceptible. The same change is produced upon the seed, when steeped in water, and laid in heaps to vegetate; and if the vegetation is stopped at a proper period, the whole farinaceous parts of the seed change into sweet matter, which, like the native sweets, may be made to undergo the vinous fermentation, and be changed into spirits. This artificial sweet matter is in its greatest perfection when the vegetation has proceeded so far that the plume has become as long as the seed, and begins to penetrate the scarfskin at the end opposite to that where the corculum is fixed to the cotyledon.

Many have thought, and still think, that the powers of vegetation only can produce this change

in the farinaceous matter, and have been disposed to consider the growth of seeds and the generation of animals as being each the produce of fermentation, though of a different kind from any I mentioned in the former part of this essay. But whether we consider this alteration in the farinaceous matter as a distinct species of fermentation or not, certainly the powers of vegetable life are no way concerned, or necessary to it. It is not during the growth only of the seed that this change can be effected; but a quantity of the sweet matter produced by the growth of the seed mixed with a quantity of the same seed ground into powder, and the whole mixed with a proper quantity of water, will all become sweet, and fall afterwards into the vinous fermentation, and be changed into spirits in the same manner as if the whole had been previously altered by the vegetation of the seed. Were it not for this property of the farina, great loss would frequently be sustained by the farmers in unfavourable seasons; grain that has once begun to grow, and whose vegetation has been stopped, can never be made to grow again. Such grain never can undergo any further making; when grain has been made to grow in this improper manner, it can hardly be supposed that the change into saccharine matters is perfect or complete. It, therefore, would be less proper for the vinous fermentation, and would furnish a smaller quantity of spirit than grain which had been perfectly malted. This grain, however, when mixed with a quantity of

perfect malt, and fermented, furnishes as much spirit as if the whole had been in the state of malt. The persons in this trade even prefer it to an equal quantity of malt; for in good seasons, when no such half malted or half spoiled grain can be got, they take good grain, reduce it to meal, and mix it with their malt, and are satisfied that they obtain more spirits in this way, than from an equal quantity of good malt. Those who follow this practice are very good judges of this circumstance, and indeed, it is otherwise so easily ascertained, that we can hardly suppose them mistaken. If one was disposed to alter the ancient system of fermentation, I should be a good deal inclined to add a new stage to this process, and say, that fermentation consists of four stages, the saccharine, the vinous, the acetous, and putrefactive. But it is rather too soon to propose this alteration, though what has been mentioned may serve to shew that certain mixtures of farinaceous matter with sweet matter may be wholly changed into saccharine matter, and the sweet matter with which it is mixed might be considered in the same light with yeast in the vinous fermentation. But many things are wanted before this opinion can be supported by experiment.

When the sweet matter mixed with farina is small in proportion to it, the whole falls easily into the state of vinegar. Hence, the same principle, which in favourable seasons, when the sweet matter is in great quantity in the grape, contributes to the strength of

the vine, in a bad season, promotes its acidity. It may, probably, also happen, that from want of proper maturity, the mucilage in the grape may sometimes be in too large proportion, the quantity of saccharine matter being no greater than its usual quantity; but this is mere conjecture, and takes place very rarely. When this is not the case, when the only fault in the grape is, its being too watery, from too much rain in the end of the season, this may be cured in part, by taking the juice after it is squeezed, and depriving it of a little of its water by exposing it for some time to heat.

The sweet matter thus concentrated, may be mixed with some juice in its natural state: the whole thus becomes fitter for producing a strong wine. This operation is often performed, even when the vintage is favourable, and the juice of the grape sufficiently sweet. By this means, a stronger and sweeter wine is produced than the juice of the grape in its ordinary state could have yielded. The part of the juice which was deprived of some of its water by heat, ferments with greater difficulty, the wine retains a sweet taste for a longer time from this cause, and this taste, by very simple operations may be retained for a very long time.

The concentration of the juice of the grape by fire is chiefly applicable to the preparation of sweet wines; those that are valued for their fine flavour and sharpness cannot be prepared in this way in any tolerable degree of perfection. The sweet matter



in the grape being in greater or smaller quantity, in proportion to the other parts, and varying in quality as well as quantity in different soils and seasons, and having its properties changed by fire, occasion great differences in the wine. It may also be added, upon this subject, that even in most favourable years, it never happens that all the grapes in a vineyard, or even all the berries in one cluster of grapes acquire the same degree of maturity at the same instant. Great attention is necessary to separate those that are not perfectly ripe, and those that are any way injured from the rest. The more accurately this is performed, the finer will the wine be, and from this circumstance, great varieties are produced, and the wines from vineyards in the same soil, and otherwise managed in the same manner, differ as much from one another, as the wines of different seasons and soils.

Besides these causes of differences in substances that have undergone the vinous fermentation, another may be mentioned, the effect of soil and climate upon the odorous principle of the grape. In this, as well as most fruits, which have been much cultivated, there is a vast variety produced by culture. Though all of these are composed of nearly the same principles, small differences are perceived. The smell and flavour differ remarkably in different individuals. The liquor, after fermentation, retains in a great measure the original flavour of the fruit. This is the chief cause of the delicate differences

among wines. Proprietors of large vineyards have an opportunity of producing more high flavoured wines than the proprietor of a small one. Their wealth and connections give them opportunities of collecting every species of vine which has been esteemed for its flavour; they even try to obtain new varieties. At the same time, by mixing grapes differing in flavour, a wine is often produced superior to what any single species could have yielded.

Besides the causes already mentioned of the differences in bodies which have passed through the vinous fermentation, several others might be enumerated. I shall only mention two. The first is, the cause of the different colours in wine: the other the cause of their harshness, their sweetness, their sparkling, or not sparkling in the glass. There are two species of grape, of which the colour only is attended to, the white and the red grape. Originally, perhaps, white wines were made from white grapes only, and red from red grapes. At present red wine is sometimes made from white grapes, but all white wines that are valued, as well as all the admired red wines, are the produce of the red grape. The juice of the red grape is as colourless as that of the white. The colour resides in the skin. It is easy to prevent the colouring matter of the hull from affecting the juice, by pressing the grapes gently and taking care to do this without their having been exposed to the sun after being pulled and picked. If the juice is wanted of a red colour, the sun

allowed to beat upon the pulled grapes before they are pressed. Some of the skins are allowed to remain mixed with the juice after it is squeezed out, and sometimes, though rarely, the skins are boiled in water, and the water mixed with the juice, or the skins are boiled with a part of the juice, and then mixed with the rest. According to the proportion of colouring matter that has mixed with the juice, the wine is either almost as colourless as water, or of a deep red, and all the intermediate shades may in this manner be produced from the red grape. White and red Champagne are made in this manner from the same grape, and the still more admired kind, the *yeux de Perdrix*. When red wine is to be made from the juice of white grapes, some colouring drug, as turnsol or archil is necessary to die the juice of the desired colour: but this is a bad practice, is now laid aside almost entirely, and can hardly be ever useful, as the red is as hardy as the white grape. The sweetness or harshness of wines, though it sometimes depends upon the watery parts of the juice having been forced away by fire, is sometimes produced by checking the fermentation before it is completely finished, and putting the whole into casks, in which one half of an inch of a brimstone match has been burned. The fermentation stops, and the liquor retains all the sweetness it possessed, when this operation was performed. Such wines are called matched wines, but are not so much esteemed as those whose sweet taste is owing

to the cause mentioned above. By stopping the fermentation before it naturally ceases, various differences in wine are produced. I need only mention that the most remarkable of these, the sparkling quality, is owing to the fermentation being checked, and the liquor put into the bottle, and the fermentation being completed without the gas, which is extricated, being allowed to escape, which of consequence is obliged to combine with the watery part of the wine. Vinous fermentation produces similar differences upon the sweet matter of other fruits as upon the grapes, and similar differences are observable in the liquor produced by fermenting many other species of sweet matter, as sugar, honey, molasses, and malt.

Though, however, these differences do actually take place, yet they are not so much attended to, as the difference arising in the grape, from the causes already mentioned. Wine is made principally for the purpose of being drank without any farther preparation. The sweet matter of sugar is fermented with a view to the spirit, which will be furnished by a subsequent distillation. The sweet matter of malt is fermented more on account of the uses to which it is applicable in the state of ale or beer, than on account of the spirit which it yields by distillation, and therefore furnishes a farther proof of the application of the preceding principles. Even, however, in the instance of sugar or honey, they may be perceived. When the juice of the sugar-cane is fer-

mented in the state it is in, as obtained by pressure from the plant, a more compounded sweet matter is obtained than pure sugar and water. For besides these a little oily or fragrant matter is obtained, mixed with viscid farinaceous or extractive parts. The fermented liquor partakes of the original qualities of those substances, and some of them accompany the spirituous part separated by distillation. Hence this spirit is of a more fragrant kind than that made of the refuse of the sugar boiler. When the juice of the sugar cane has been boiled, and sugar separated from it, the refuse, the molasses and scummings, furnish a spirit less fragrant.

But no differences can be perceived in this way, excepting such as arise from substances that are volatile, the rest remain in the liquor after the spirit is separated, and pass unnoticed or disregarded. In some cases, however, fragrant substances are mixed with sugar and water, in order to furnish a liquor more agreeable than pure sugar would produce, and therefore to be used in the vinous state upon the principle, that the fragrant matter will remain after the fermentation. All kinds of made wines belong to this head, and though in this country they are looked upon with contempt, yet they are as wholesome, and most of them as palatable, as those produced from the grape. Of all the sweet substances fermented for the sake of the spirit, the one that contains the greatest mixture, is that from which arrack is made—the juice of the palm tree, and the



milk of cocoa nut. The first of these is compounded like the juices of other sweet plants; the latter contains, as I have found by analysis, a quantity of sweet matter mixed with oil and farina, and something of a narcotic substance, but the last is in very small quantity, though sufficient to make the milk, when freely drank, produce dangerous, and almost fatal effects. Were we to examine the juice of the palm and the milk of cocoa, immediately after fermentation, we should find a very compounded and disagreeable liquor; by distillation these are partly separated; much oily odoriferous matter distils from the spirit, and serves to distinguish it from all other fluids. Malt is chiefly used for the sake of the vinous liquor which it yields by fermentation.

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## ESSAY IX.

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### *ON FERMENTATION.*

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**I**N a former essay I considered fermentation in general, and the various means of promoting or retarding it. I mentioned the three stages into which it has been generally divided; the vinous, the acetous, and putrefactive. I took notice of the general disposition in bodies to fall into these stages; that sweet substances are capable of being changed in the greatest variety of ways by fermentation, and therefore that the nature of fermentation can most easily be discovered by an attentive observation of the sweet substances in the various stages of this process; that the sweet matter in fruits was probably first fermented, and is of the simplest kind. This I illustrated from the nature of the process for making wine, and pointed out the causes of the differences

among wines. They all consist of the same substance fermented in nearly the same way. I said that the nature of fermentation is but little understood, in so far as the causes of the changes produced by it are concerned; that it was not my intention to give any theory of fermentation, but to give an account of the phenomena, to prepare us for the theory in due time.

The effects produced by fermentation are more easily observed when the liquor fermented is used immediately, or at least without any further operation. On this account I shall not mention at present the changes produced by fermentation upon sugar, and other purer sweets, but confine myself to the fermentation best known in this country, and endeavour to mention some of the causes of the differences produced by fermentation upon the sweet matter in malt.

The fermentation of grain furnishes greater variety than that of any other substance. This is owing partly to the farina it contains, and to the changes which this farina may be made to undergo by vegetation. The seeds of most of the culmiferous plants are nearly of the same nature, and all of them may be fermented into a vinous liquor. They all contain, besides the rudiments of the young vegetable, a quantity of a substance to nourish and support it, till it acquires sufficient strength to prepare food for itself. The part chiefly employed is

farina, sometimes mixed with more, sometimes with less of a sweet matter. Farina is an insipid inodorous substance, not soluble in cold water, and forming with hot water a glutinous mass. It is never found quite pure in the seeds of plants. It is mixed not only with vascular parts of the young vegetable, but united sometimes with a gluten, sometimes with sweet, sometimes with oily, or bitter, and narcotic matter. The farina is extremely nourishing to animals, and it is not improbable that the nourishing quality of all vegetables depends on the portion of it which they contain.

Those seeds which contain least foreign matter mixed with farina, are preferred to the rest, as they yield not only the most agreeable food, but are likewise most capable of being applied to useful purposes after fermentation. Hence wheat, barley, and oats, and some others, are preferred to the rest. When the farinaceous part is quite pure, it is hardly capable of being fermented at all, but is the most incorruptible of all vegetable substances. When mixed with sweet matter, it may be fermented in many different ways, according to the manner of managing it. The farina in the seeds of plants is always mixed with sweet matter, and may be used for the purposes of life, in one or other of these two states. These seeds may either be fermented in their natural state mixed with a little water, or they may be fermented after they have begun to vegetate, when mixed with much water. In the first case

they are used as food, and do not acquire the properties of wine; in the latter, they fall into the vinous fermentation, and lose the greatest part of their nutritive quality. The farinaceous seeds ground into powder and exposed to the circumstances favourable for fermentation very readily become sour, or acquire the properties of leaven. If mixed previously with a quantity of any ferment from a vinous liquor such as yeast, a real vinous fermentation takes place, the viscosity of the farinaceous matter is diminished, and the whole becomes more spongy; water penetrates it more easily than before, and it mixes more readily with any watery fluid which is taken along with the food. During the fermentation a quantity of aerial matter similar to that from fermenting wine separates. The mass does not acquire any intoxicating quality, from the farina being in a state different from the sweet matter, and the quantity of the latter being too small to produce a complete change upon the whole. There, however, can be hardly a doubt that a farinaceous seed, when fermenting in the manner just related, would really yield some ardent spirit: though no attempts that I know of have been made to collect it. The fermentation is never allowed to proceed far, lest the mass should become sour, and the whole is exposed very suddenly to a considerable degree of heat, to stop the fermentation completely as soon as possible.

When the seed contained originally but little sweet matter the fermentation seems to be less of the vinous

and more of the acid kind, and in many instances seems to be of the acetous nature altogether. The farina in the seeds of vegetables is changed by vegetation into a sweet substance easily miscible with water. By this means they are converted into malt. But the change of the farina into sweet matter by vegetation is not in proportion to the original sweet matter mixed with it. Wheat and barley are instances of this : the first contains most sweet matter in its natural state ; the other having vegetated. Vegetation seems in this instance to change the nature of barley more easily into sweet matter than it does the farina of wheat, and is capable of producing more sweet matter from the one than it can from an equal quantity in the other. The cause of this is not easily assigned ; the fact, I believe, is indisputable, and the solution of the difficulty will be found to depend upon the mixture of a substance with the sweet and farinaceous matter in wheat, and which, when separated, is of a glutinous quality, and will neither dissolve in hot nor cold water, and has some of the properties of animal substances.

Vegetation not only affects the farinaceous matter differently in different seeds, but produces different effects upon the same seed, by which the nature of the farina is more or less altered according to the different degrees of perfection to which the seed has arrived. Much depends upon its degree of maturity. When the soil and season are good, the quantity of sweet matter produced by vegetation is greater and purer,



and it is produced in less time. Hence it is found necessary to keep the grain of different soils separate. Were they exposed to the malting process together, it would be over in one kind, before it was well begun in another, and if continued till it was complete in the latter, the former would be spoiled. The same attention is necessary in grain of different years. The longer the grain has been kept, the more time is necessary to change the nature of the farina contained in it.

Even when the grain is all precisely of the same quality, vegetation in different degrees or differently conducted, will alter the quantity and quality of the sweet matter. Many still more minute differences might be mentioned, which would be found useful in accounting for the smaller differences produced afterwards by fermentation. The perfection of the sweet matter in grain depends upon bringing the seed to a precise point of vegetation. If it is carried beyond this point, the sweet matter is changed into a state unfavourable to the production of a perfect vinous liquor; if it is short of it, a part of the farina remains in its natural state, which, if not unfavourable to fermentation, at least diminishes the spirit in it. In some instances, however, the vegetation is stopped before the whole farina is changed into sweet matter, in order to obtain afterwards by fermentation a milder and more viscid, though less spirituous liquor. When this is intended, the grain is just allowed to begin to grow, and the moment the radicle makes



its appearance, the vegetation is stopped. When a thinner, lighter, harder, and stronger liquor is wanted, the vegetation is allowed to proceed farther, and is not completely stopped till the plume has become as long as the seed. In this case, the sweet matter in the grain is in greatest quantity, and has acquired the properties best fitted for undergoing completely the vinous fermentation. In all cases, however, the sweet matter ought to be in the same state in the whole grain, upon which the malting process is performing. To render therefore, the vegetation equal in every grain of which the heap is composed, is the great object in view, and this is attempted by frequently turning it, that every grain may be equally exposed to the air, and the whole brought to the desired point if possible. When this is accomplished, the whole should be withered, by spreading it thin on the floor and drying it as soon as possible. There is one circumstance worthy of notice in this process, which has not been mentioned by any writer, and which I imagine, is not so much attended to as it ought to be, even by those whose profession it more particularly regards, I mean the quantity and equal distribution of the light in the apartment where the process of malting is carried on. It is known to every body that light is of the utmost importance in vegetation, that vegetables will not grow in a dark place, at least that they do not arrive to any degree of perfection in this situation, and never contain any thing but an insipid

watery juice ; and that all plants growing in a dark place have juices of the same qualities. If a ray of light is admitted into the dark apartment, the vegetable grows with greater vigour, and directs itself to the aperture by which the light entered. Even different degrees of light produce sensible differences; vegetables growing in a situation most exposed to the sun have the richest juices, and this is in no case more remarkable than in the production of sweet matter, as seems to be evident from the sweet matter in grapes on the sunny side of a hill, being in greater quantity than in grapes of the same species gathered from the north side of it. One might expect that the sweet matter in grain beginning to vegetate should also be affected by light. The seeds of plants growing below ground may indeed be supposed to be little affected by light : but grain in malting must be affected by it. Even the seed below ground is not altogether out of the reach of the influence of light, as is evident from comparing the growth of seeds in different pots of earth, some of which are exposed to the light, and others kept in a dark place, the heat in all being equal. It is a common observation, that the sweet matter in malt and the other liquors made from it, is subject to great variation. No doubt this is in part owing to the differences betwixt different ears of barley in the same field, or to differences arising from moisture, or the different degrees of exposure to the air. But it seems reasonable to suppose that part of those

differences arise from the different degrees of light to which the grain has been exposed during its vegetation: In most cases the light admitted is not great, but still uniformity in this respect would probably be of advantage. I suppose, one reason why much light is not admitted in the malting process, is that the vegetation would proceed too rapidly and be more difficult to stop at the proper point. I should wish likewise the experiment were tried to ascertain what would be the effect of a perfectly dark place upon grain beginning to vegetate, in so far as the nature and properties of the sweet matter contained in it were concerned.

Besides the differences produced by fermentation upon grain in its natural state, and after being malted, other differences arise from the operations it is exposed to, or the additions made to it before fermentation. Of the differences arising from alterations produced by operations previous to the fermentation, the most remarkable are produced by heat. This must always be employed after the grain has vegetated, in order to put a stop to the vegetation at that point when the sweet matter is in greatest quantity, or best suited to the kind of liquor to be made from it by fermentation. If the water which the grain absorbed in the operation previous to its malting, be allowed to remain after that process is over, the sweet matter will soon be diminished, and acquire a strong tendency to the acetous fermentation. In all cases therefore the grain must be dried to preserve it in its most perfect state for

the purpose intended. There are two ways of doing this; one by exposing the grain to an excessively hot sun, which soon dries up the moisture without producing any material alteration upon the other parts. In this manner the colour remains unaltered; and when water is afterwards employed, a very pale infusion is obtained, and after fermentation the liquor differs but little in colour from water. When the vegetation is checked by heat, raised by fuel, the sweet matter is always more or less altered, and the scarf skin of the grain becomes of a darker colour, and the whole communicates to water a deep yellow or brown colour. Something likewise depends upon the kind of fuel used. If the fuel employed has any smoke, a part of it condenses upon the grain, and communicates a taste to the infusion and to the liquor after fermentation. The greater the degree of heat employed, the darker coloured is the infusion. It is, however, possible to employ fuel, if it has no smoke, in such a manner as not to affect the colour of the infusion. By throwing a little sulphur upon the fuel the malt will give a paler colour to water. If sulphureous coals are employed the same effect will be produced; but the fermentation of the infusion will proceed in a very different manner from the effect which the acid of the sulphur communicates to the sweet matter. This practice seems to be a dangerous one, and can hardly be executed with any prospect of success.

Besides the effects already mentioned, the greater the degree of heat to which the grain has been

exposed, the more difficult will it be to bring on the process of fermentation: and it may happen from this cause that it will become altogether impracticable. To this head also belong the effects depending upon the heat of the water by which the sweet matter is extracted. One circumstance must particularly be attended to, that the heat be not so great as to reduce the malt into a pulpy state, from which the water will not run. This will certainly happen if the water be poured boiling to the malt ground into powder, and even when bruised, which is done to render this less liable to take place. Besides the difference in the heat of the water, the time it is allowed to remain upon the malt, and the precautions used to preserve the heat, occasion several alterations in the liquor produced by fermentation. The more agreeable parts are extracted first; if the water is drawn off as soon as the heavy parts are deposited, a more agreeable liquor will be obtained, and the sweet matter remaining may be extracted by pouring more water and employing this second infusion to inferior purposes. The great delicacy of the business seems to depend upon the heat of the water. No part of the operation is less generally known, and in none has the great manufacturer so much the advantage over the small one. When great quantities of materials are used at a time, they preserve their heat longer, and by this means a more moderate heat is capable of producing a greater effect. The thermometer has been called to assist in this part of the

operation, instead of using the hand of the brewer as a measurer of heat. The degree of heat most favourable for extracting the sweet matter, without making it into a pulp and without injuring the sweet matter or the subsequent fermentation, seems to be the great secret, and is anxiously concealed by the people in the trade. They even conceal it from the thermometer makers, by substituting letters for numbers to mark the degrees in their thermometers. So far, however, as I have been able to learn they use different degrees of heat for different purposes, and that from 163 to 168 of Fahrenheit, is most generally employed in making the finer species of malt liquors. It happens too, very frequently, that differences in the liquor after fermentation arise from the time the infusion has been boiled before this process is begun. This is always necessary to prevent, it is said, the danger of souring, and also to condense the infusion so as to furnish a stronger liquor. It is also employed to mix substances with the infusion to preserve the liquor after it is fermented; how far it is necessary on the first of these accounts is not known; perhaps the substances mixed commonly with water may render it necessary. Something may depend on substances contained in the water. Soft water is supposed in general to be preferable to hard, and the water of certain rivers, like the Thames, superior to all others.

The differences produced by addition previous to the fermentation deserve also to be mentioned. Of these the most common and most useful is the



addition of bitter plants to the infusion of malt. The plant now most generally preferred is the hop, a substance of an intense bitter, mixed with a little aromatic matter. This is added with a view chiefly to preserve the liquor from becoming sour after it is fermented, to which every malt liquor is very subject. With this intention the hop is boiled in the wort. This seems, however, to be ill calculated for the purpose. By this means the aromatic part is lost, which might easily be retained by steeping the hop in a close vessel in some hot water, and afterwards mixing this with the rest when removed from the fire. Some may perhaps imagine this to be injurious : but it has been tried by many at my request with success. The bitter taste communicated by this vegetable gradually diminishes, and at last ceases or dies. This is owing to the gradual formation of an acid in the fermented liquor, and any bitter substance may be instantaneously deprived of its taste by adding a sufficient quantity of acid to it. This I have long employed as a test of the quantity of bitter matter in bodies.

Sometimes common salt is added to the water intended to be used in extracting the sweet matter of malt. This addition, it may be supposed, is made with a view chiefly to excite thirst; but besides this effect it produces also some others, particularly it moderates the fermentation, makes the liquor fine, and seems to be the cause of the great superiority of the water of certain places in producing fine malt liquor. The water of the Thames

is remarkably celebrated on this account, and it contains a little salt; and in most places remarkable for fine malt liquor, I have found this also to be the case. To answer these purposes the quantity of salt contained in the water should be small; no advantage will be obtained if the quantity is so great as to be discovered by the taste.

Besides these causes of differences in malt liquors there is one less generally attended to, arising from the different nature of the grain in different soils, and seasons which occasions varieties in the liquors produced, though the proportion of grain and water be the same. This principally affects the strength of the liquor, and it were to be wished some accurate means could be contrived to make the water and sweet always bear an accurate proportion to each other. It has accordingly been lately proposed to accomplish this by means of an instrument which will shew the quantity of matter extracted by water from the grain without its being necessary to know what quantity of each had been employed. The instrument is constructed like the common hydrometer, is made of glass or metal, and points out by the quantity of the instrument which sinks, how much the fluid differs from pure water. The less it sinks, the more has the water dissolved, and the stronger it is of the soluble matter of the body on which it was poured. This instrument, when put into an infusion of malt, shews how much has been dissolved, and as the soluble part in malt is of a

saccharine nature, the instrument has been called a saccharometer. It is, however, difficult to make a scale to it, so that observations made by different persons may be compared, and it is liable to some variations from the heat of the fluid in which it is immersed varying, as well as from differences in the matter dissolved from the causes already mentioned. With some attention these might probably be lessened or removed, in which case it would prove of considerable utility. Some of the differences in fermenting malt liquors also depend upon the manner in which the fermentation has been conducted. The sweet matter in malt does not fall readily of itself into the vinous fermentation. Even when exposed to the most proper degree of heat, in the most favorable circumstances, it is more disposed to become sour.

The various heterogeneous substances mixed with the sweet matter in malt, together with the heat employed in drying it, probably occasion this tendency and afford a reason why little artificial heat is necessary for the perfect fermentation of liquor made from malt. This is so much the case that in warm climates and seasons it is hardly possible to produce tolerable malt liquor; and to the same circumstances it is not improbable we owe the superiority of that made in October and March, and distinguished by the names of the months in which it was made. In the most favourable seasons and circumstances some fermenting substance must be added to dispose

the whole to the vinous state. And this generally consists of the scum which rises from liquors in the state of vinous fermentation. How this substance acts is not easily explained, but some of its properties are known. It seems to be of a farinaceous nature, and is easily preserved with all its properties in a moist state for many months, or small twigs dipped in it and dried will retain the property of exciting the vinous fermentation for a long time. It has indeed been asserted that yeast is nothing more than a substance strongly impregnated with fixed air. But this can scarcely be imagined to be the case when it is preserved in a dry state. Though the vinous fermentation is produced with difficulty in infusions of malt, yet if once raised, it proceeds with great violence and is very apt to proceed beyond the proper boundary. The fermentation is on this account always checked before it is completely finished, and the remainder of the process is performed in a very gradual manner, and often takes years in being completed. To know when it is proper to check the fermentation is a circumstance of great importance. No other mark has been commonly used excepting the change in taste of the liquor. As this is subject to great irregularities, it has been proposed to obtain a more perfect measure of the progress of fermentation from the change produced in the density of the fluid. The same instrument which has been proposed to measure the quantity of the sweet matter mixed with the water before

the fermentation, may also be used to measure the alteration in density produced by fermentation. When used for this purpose it seems to be liable to fewer objections. It will no doubt require nice observations to find out accurately the change in density which should be produced in every case according as the infusion was strong or weak, according as it is intended that the liquor should retain some of its sweetness after fermentation or lose it intirely, and according as it is intended that the liquor should be used soon after the fermentation or kept for a considerable time after it. At whatever period it may be judged necessary to stop the fermentation, it will always be necessary to put the liquor into clean vessels, and to keep the whole for some time at least in a cool place and in close vessels. After all the operations to which the malt liquor has been subjected, we shall find it more compounded than wine, and that it will retain much viscid matter, which was originally mixed with its sweet matter, together with some vinegar which has been formed from the action of the viscid parts on the sweet matter during the vinous fermentation, by which means the liquor, though more nourishing than wine, is not so light on the stomach, is more heating, and is best fitted for the constitution of laborious people.

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and the other side of the road  
the other side of the road  
at the end of the road



# ESSAY X.

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## *O N C E M E N T S.*

Read A. D. 1782.

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**C**EMENT in building was probably used soon after human habitations began to be built of stone. Without some invention of this kind it is evident, that houses could afford but a very imperfect defence against the weather, and though they might be tolerable in a very warm climate, they would furnish an uncomfortable lodging in a cold one. What substances were first employed to join stones together in building, it is, perhaps, at present impossible to determine with precision. It seems, however, reasonable to suppose that the substances first used for this purpose were possessed of the more obvious properties of cements, to wit, tenacity and ductility when wet, with a considerable hardness when dry. The body which possesses these qualities most remarkably in a state of nature is clay. When mixed •

with a little water, it is extremely tenacious and ductile, capable of being spread to almost any degree of thinness, and when the water is separated from it by the spontaneous heat of the weather, it becomes hard, and makes the substances between which it was spread cohere with a small force. As the clay however, is liable to be penetrated by water in very wet weather, it will be apt first to be reduced to its original soft state, and then to be washed away by the rain. For both these reasons buildings in which clay is the cement can have but little durability. Besides the inconveniences just mentioned, clay, when mixed with water, is apt to crack in drying, by which means a great number of crevices will be formed in the building in which it is used as a cement; through these the wind and rain will freely pass, so as to render the house extremely uncomfortable during winds; and in wet weather, the rain being more freely admitted, will occasion damp, and effectually contribute to the destruction of the edifice, the cement itself being quickly washed away.

The inconveniences of clay as a cement, from the cracks which are formed in it on drying, might be remedied by mixing such substances with it as are least penetrable by water, and which of consequence are less apt to have their dimensions altered by drying. Sand is of this kind; but a mixture of clay and sand has less tenacity than clay, and is more easily penetrated by water; though as it does not crack in drying, it is still frequently employed in coarse

buildings. Of this kind also are most dry vegetables, particularly such as having arrived at perfect maturity by completely ripening their parts, have little water in their composition. Annual plants, in which the vegetation ceases in the roots as soon as the seed is ripe, have less water in them than other plants in the same situation, and are therefore less liable to alter their dimensions by being exposed to a dry air. These substances mixed with clay would remedy in part the inconveniences arising in drying. Straw or stubble possesses the properties alluded to in the greatest degree, and would probably be used in mixture with clay when intended to be employed as a cement. If these could not be had, the withered leaves of trees might be substituted for them. Such mixtures of clay and dry vegetables have been used as cement in building among many nations, and are still used by the more uncultivated parts of our own. The buildings thus constructed are, however, neither elegant nor durable. And though the addition of vegetable matter may prevent the cracking of the clay by the separation of the water, yet that very matter is subject to decay in a certain time, and, by the gradual dissipation of parts produced by putrefaction, to lose all the properties for which it was originally used. By these means the cement in a certain time becomes more porous, and more easily penetrated by the wind and rain, as well as more liable to scale off, than if no such addition had been made.

The inconveniences of this cement must soon either have made mankind lay aside stone in building, and have recourse to their old material of wood, or try by various experiments to discover a cement more perfect than that made of clay. It is supposed by some, that fire was used to make the stones in buildings cohere, that houses in some places were made of a very fusible kind of stone, that afterwards a great fire was kindled in the house sufficient to melt, at least to soften the stones of which it was made, by which means the whole stones cohered and became as one piece equally impenetrable to air and water. Such, it is said, were some buildings in Scotland, which have been described under the name of vitrified forts. Concerning these it may be observed that, granting the fact, this, though a very durable, would be a very expensive mode of building. It would be uncertain in many cases, apt to fail either by too small or too great a degree of heat; too little making the joining imperfect, and too much distorting the figure of the building. Nothing elegant could be produced by this mode of operating, and the process would fail altogether excepting when the stone was of a very fusible kind. A house of marble treated in this manner would crumble into powder. The joining of freestone and granite would be as imperfect after this operation as before it, and the only species of stone in which it could at all succeed is whinstone, and some others like it. Perhaps very bad bricks might be joined in this

manner. If the walls were of any great thickness the inside would be in the state of glass before the outside had acquired a red heat. This method of cementing stones in building must be subject to numerous inconveniences even in the most favorable situations, and in many would fail altogether.

Buildings of this kind are, I believe, seldom to be met with but in high grounds, or on the tops of hills, where the wind, blowing with more violence, might raise the fire to a greater degree than could easily be done in a lower situation. In this respect the vitrified forts resemble the furnaces which our ancestors constructed on the tops of hills for the purpose of smelting metals from their ores, before the mechanical arts had arrived at such a state of improvement as to admit their being constructed in less elevated and more convenient situations.

As the practice of cementing stones by means of fire seems never to have been very general, other means have been fallen upon by which the stones in buildings might be more conveniently and as effectually joined. The substance which has been most generally employed for this purpose is the calcareous earth in the state of lime mixed with certain substances, and reduced to a proper consistence by means of water. This mixture, when properly made, produces the best cement hitherto used in building. But its qualities differ very much according to the preparation which the calcareous earth has undergone, and the substances that have been mixed with

it. The calcareous earth in its crude state is of no use in building. When reduced to powder and mixed with water, it has no tenacity, and upon the separation of that water is equally dry and void of cohesion. When the calcareous earth has been exposed to the utmost violence of fire for a long time, it becomes very brittle, loses considerably of its weight, becomes very hot on throwing water on it, and falls into a bulky powder, which is, however still very dry. Upon mixing more water with it, it forms a paste possessed of a considerable tenacity, and this by long exposure to the air becomes gritty in its particles, but does not cohere so as to form a solid mass. It consists of a number of hard particles, but has no properties that would make one use it as a cement in building. When a little lime is diffused with a great deal of water, it dissolves and forms lime water; and by exposing this water to the air in a glass, there forms a very hard crust on the vessel which separates from it with difficulty, and seems to have all the hardness which the calcareous earth can acquire. If one could give the whole mass the tenacity and hardness which the little contained in lime-water is capable of acquiring, it would possess all the qualities of a good cement.

When we examine into the causes of this difference in the same substance exposed to air in these two ways, we shall find that the want of cohesion in the large mass is principally owing to the altera-



tion in size by the separation of water, and attraction of part of the air in consequence of exposure. If one could remedy the inconveniences arising from this source, a perfect cement would be produced, provided the calcareous earth was originally sufficiently pure, and the degree of heat to which it was exposed had been sufficiently violent. A very small quantity of lime is sufficient to give tenacity to a great quantity of other materials. It has been found that lime requires some additions to fit it for a cement, and these are to prevent cracking by the separation of water, which all cement must contain, and to enable the lime to absorb air, and to have that chemical effect produced on it, by which it becomes hard and capable of making the stones cohere. The cracking of the cement arising from the alteration in its bulk by the separation of water may be prevented in several ways. The most common is to add hair if the cement is to be applied to the inside of houses, and sand or siliceous earth in the body of the house and where it is exposed to weather. Sand, in the composition of cement, answers some other purposes besides preventing cracking from the dissipation of the water. Sand is perfectly insoluble in water, and is incapable of being acted upon by the lime, it does not alter its dimensions at all when made an ingredient in cement, and therefore occasions a less alteration than would have taken place had the whole mass consisted of lime. That sand may answer this purpose it is necessary it should be pure, and par-

ticularly that it should be perfectly free of clay or mud, both of which imbibing water and altering their dimensions by it are subject to the same inconvenience that lime itself is, and to remedy which sand was added. River sand, which has less mud or clay than pit sand, is universally preferred as an ingredient in cements for building.

A proper mixture of sand and slaked lime in process of time becomes hard without cracking, the hardness increases with age, and it is impervious to water. The hardening of the cement in building is entirely owing to the effect of the air on the lime, and not to the separation of water from it. By recovering that air which the fire had separated, it recovers, in a great measure, the hardness which it had before burning. The sooner lime can recover this air, the quicker will the cement harden; and the more equally the air is furnished to the lime, the more uniform will the mass be, and the greater the cohesion of the whole. That the air may be easily and equally furnished to the lime, it is necessary that the lime should be equally and perfectly calcined, and the cement made of it be as equally exposed to the action of the air as possible. This is effected first by using the lime as soon after it is burnt as possible, and secondly by mixing sand or other substances with it in sufficient quantity and of a proper size. That the using lime new-slaked and as perfectly burnt as possible, will greatly contribute to the hardness of the cement, is evident from this con-

consideration, that it is only when newly and perfectly burnt that all the lime is capable of hardening by exposure. If it is kept for ever so short a time after burning, some of it returns to its original state. Thus a part of the lime is capable of hardening by attraction of air and crySTALLIZATION, while a part of it is incapable of any alteration, and is of no more service than an equal quantity of chalk or unburned lime. By these means a part of it is not only useless, but by screening the perfect part from the action of the air, it retards the hardening of the cement and tends very much to make it scale off.

For these reasons I think the common practice of keeping lime a long time before it is used is a very bad one. The workmen generally slake their lime, mix a quantity of sand with it, and keep it in this manner for several months, and this process they call by a very improper name, "souring of the lime." This practice is very general, and to it I am persuaded in a great measure is owing the bad quality of the cement used in modern buildings. This can be of no service but to undo a part of the effect produced by the heat upon the lime, as the long exposure to the air will furnish that matter which had been separated by the fire. There is, however, generally some good reason for any practice which is common to all the persons in the same trade, though it may not, in all cases, be easy to discover it. And it will be found that though this practice will never make good cement, it may improve bad cement

made of half-burnt lime. This bad custom seems to have been introduced by an erroneous opinion, very general, concerning the nature of the calcareous earth or limestone, used in making lime, to wit, that the purest limestone, or marble, or chalk, do not make the best cement, and workmen universally prefer lime made of the harder and more impure limestones. It is difficult to understand what first suggested this erroneous notion. It seems, however, to have been owing to the following circumstances. Chalk, though the purest species of calcareous earth, is a very soft and spongy substance. When exposed to the fire for a short time it flakes very easily, though it is not above half burned. The more impure limestones, on the contrary, after calcination, will not fall into powder upon flaking unless the air has in a great measure been expelled by the fire. The flaking of the lime is, in the last case, a proof of the limestone being perfectly burnt; which is not the case in lime made of chalk. This might make the workmen ascribe to the stone the qualities which depend intirely on the calcination.

The vulgar criterion, that the ready flaking of lime is a proof of its fitness for building, is true only in lime when made of hard stones. This seems to be the chief reason why all the authors, whom I have consulted, from Vitruvius downwards, agree that lime made from the closest and hardest stone makes the best cement. Another reason for this opinion may be, that spongy substances, like chalk,

more easily recover the air, which the fire had separated, and are more injured by keeping, than lime made of harder stones. A pound of chalk lime will absorb  $2\frac{1}{2}$  ounces of air in a day without apparently flaking. The lime of chalk may therefore be much injured without the flaking shewing that it is so. Some portion of every kind of limestone, used in this country, is not sufficiently burnt, and does not flake readily. Some parts are half vitrified, and some limestones contain marl or gypsum. Workmen would naturally be led to the practice of keeping the lime some time after it was flaked, that the water might penetrate the whole more thoroughly. In the common lime, if this practice was not followed, it could scarcely be used as a cement at all, because those parts which escaped the action of the water in the flaking, would afterwards imbibe moisture from the air, would swell, crack, and fall off. The practice of souring the lime, therefore answers no other purpose but to give time to the water to penetrate the whole of it; and as the best burnt is penetrated soonest, while the rest requires longer time, the whole is, by keeping, brought nearer to equality, but far short of that state of lime which is capable of giving the greatest degree of hardness in cement.

There is, perhaps, another reason for souring lime. Plaster for the inside of houses must be laid on very smooth, and requires to be more tenacious, and to have more lime in it than the cement used



in building. This quantity of lime would be apt to crack in drying, for the reasons already mentioned. They are therefore obliged to keep it longer, that the lime, previous to being used, may have time to absorb a quantity of air. The builders, considering the plasterers' mortar as of a finer kind, think it not improper to imitate them in those particulars, which are attended with no expence, and do not seem to know that such measures prevent their cement from ever acquiring that degree of hardness in which its perfection consists.

The proper method of using lime made of the coarsest limestones, and avoiding the inconveniencies arising from the unequal action of the fire, even in the best, would be to put the lime on a sieve, plunge the whole in water for a few moments, lift it up, and allow all the lime to flake that will do so readily, and separate this part carefully from the rest. It will always be found to be the best calcined, and will produce a finer and harder cement than could have been made, had the whole been flaked in the ordinary manner. The part which does not flake should be kept for other purposes, and the expence of this process might be diminished by employing that, which does not fall at first, as a manure for land, for which purpose it is as good as lime in its most perfect state.

Besides having lime in the best condition, it is necessary that the whole mass of cement should be so made that the lime in it may be exposed to the



air as much as possible. The smaller the proportion of lime in cement, the more surface will it expose to the air. On this account, six or seven parts of sand to one of lime, will be more convenient, than the common proportion of two or three. But so large a proportion as six or seven parts of sand to one of lime, cannot be used for cement without the mixture becoming so brittle, that it will not adhere to the surface on which it is spread. If one was to mix flints or sand in fine powder in this proportion with lime, the desired tenacity might be given to the cement, but other inconveniences would be produced. The cement thus made is never so strong as that made with coarser sand. It makes indeed a beautiful and sufficiently hard cement for finishing the inside of rooms, much harder and closer than the common stucco: but it is not hard enough to give all the firmness to the stones in a building that the best mixture of lime and sand is capable of giving. If the cement made with powdered flint is struck with a hard body, it cracks, and the fissure is always rectilinear, or nearly so. When mortar is made of coarse sand, it becomes much harder, and when struck by a hard body the crack is never in a right line. The causes of the inferiority of cement containing fine powder seem to be two, the one mechanical, and the other chemical. The first is, that, a solid mass of fine particles of unequal hardness being struck, the softest are broken; and the size of the hardest being very small, the direction

of the crack seems to be the same as if the whole had been made of soft matter, and the crack is in the straight line. But if we strike a mass of cement, consisting of fine soft particles and coarse hard ones, the fracture is always crooked, because so much of the softer matter cannot be found in a straight line, as to make the fissure rectilinear: the harder and larger particles are for ever standing in the way, and the whole mass is more difficult to break. The second reason why the mass with coarse sand and lime will be harder than fine powdered flint and lime, is, that the fine powder produces a more tenacious mass, through which air and moisture pass with more difficulty. The lime in such a mixture will therefore be longer in recovering the air it had lost, and will be longer of assuming a crystalline form. On these accounts, perhaps, fine powder of flint in cements cannot be used where greater hardness is wanted. It may be employed in ornaments in the inside of a house, but never to advantage as a cement in building.

Other methods must be tried to mix the seven parts of sand with one of lime. The best mode of mixing so large a portion, seems to be, to use sand of different sizes in the same cement: the larger pieces will give strength to the mixture, and allow the air more freely to come in contact with the lime; the finer sand in the mixture will fill up some of the interstices betwixt the particles of the coarser, and with a less proportion of lime, give a greater degree of tenacity, than if the cement had been

made of either separate. It ought further to be observed, that a little difference in the cement will arise from the shape of the particles of the sand in it, as well as from its quantity. The cement will be harder when the particles of sand are flat, than when they are round. When they are round the particles touch each other in fewer points. The cohesion of the whole will therefore be less. When the particles of the sand are flat the surface in contact is greater, a smaller quantity of lime will fill up the interstices, and less of the lime will be kept away from the action of the air. The cement will therefore not only become harder, but will do so in a shorter time, because the lime in it will sooner and more perfectly recover the air it had lost in the fire.

That sharp sand answers better in the composition of cement than round sea sand, all builders acknowledge. But few of them will admit that seven parts of sand to one of lime will produce a cement superior to that in common use. That this proportion, however, is better than the common, may be proved by examining the cement in old buildings, where, from the age of the structure and firmness of the cement, there is reason to suppose that it was of the best quality. In many old cements I have found by experiment the proportion of lime not more than one-seventh of the mass; and that in such cement the sand or flint is of different sizes, must have been observed by every body.

It ought likewise to be remarked, that a little latitude is allowable in the quantity of sand and water in the composition of cement according to the time it is supposed the cement will take to dry. Much lime and as little water as possible will answer best, when the cement is to dry soon. Less lime and more fluid will answer best, if it is to dry slowly, for the same quantity of water gives more fluidity to mortar in proportion as the sand is coarser.

Though the mixture of sand of different sizes with lime and water in the proportions already mentioned, produce a good cement, it is very long before it acquires its utmost degree of hardness; and this is its greatest fault. The older it is the harder it grows. But the first year it is liable to be penetrated by rains, and, if frost succeeds, to scale off by the expansion of water in freezing. The failing of the common cement, as well as that above mentioned, is principally owing to this; to remedy which, many persons have proposed to use something besides sand and lime in the composition of cement.

The most celebrated cement upon this principle was proposed by M. Lorient, and consisted of a quantity of powdered unslaked lime mixed with a cement made of sand and common chalk or whiting. The mixture was to be used immediately. But this composition, though very much extolled by its author, has not come into general use. He

seems to proceed on the mistake that the drying and hardening of the cement are the same thing ; though the one depends on the separation of the water, and the other upon the absorption of a quantity of air, and consequent crystallization. M. Lorient's cement does, indeed, dry very fast by the unslaked lime soon absorbing the water in the cement. But it is still very far from possessing the hardness of old cements, and if the unslaked lime is unequally burnt, it continues to swell, and in a short time falls off, as I have found by repeated trials, as well as that the addition of old lime or chalk or whiteing is never of any advantage, and generally does harm. The proportion of sand in Lorient's cement is too small.

Others have recommended skimmed and buttermilk or blood to be used instead of water in mixture with lime and sand in the composition of cement ; and many are of opinion that these substances contribute to the quick hardening of the cement. But in all mortars which I have seen, they seemed to be of no service, and always gave a disagreeable appearance, when used in casting of houses, a vegetation being produced in the cement, which by keeping it moist tended to spoil it. Decoction of linseed, though strongly recommended, has the same inconveniences, and all of them produce no advantage, except making the cement more glutinous than it would have been with pure water ; but it is



as liable to crack as the common cement, and the only service they can be of is to give an equal tenacity to a less portion of lime in the mixture. Linseed and olive oil mixed with lime produce a sort of soap, which is long in drying, and seems to have no properties of a cement. Mixtures of linseed oil, chalk, and sand, in dry situations, acquire a great degree of hardness, and are sometimes used as a cement in building, they differ little from common putty, but the oil spoils in time. Painting of cement prevents the further hardening by cutting off the communication with air, and wherever water can get behind the paint, irreparably destroys it. Charcoal of wood, and charred pitcoal powdered and mixed with lime and sand, seem to make the cement harden faster; but the difference is not very great: the cement by this mixture acquires a bluish grey or slate colour.

Some have recommended clay in the composition of cement containing lime and sand. But in all mixtures of this kind the cement is much injured by the addition of clay. It is more disposed to crack in drying, to soften in wet weather, and even to moulder, if the clay be one-eighth of the cement. In a smaller proportion it only prevents the mortar from acquiring the hardness peculiar to it, and disposes it to perish in a few years. Besides clay adheres but very weakly to any hard bodies, however slowly it may be dried on them; and mixtures of



sand and clay never acquire any considerable hardness by mere drying and exposure to the air. As dried clay greedily imbibes water, and contracts greatly in the drying, it must be injurious in cement.

The substance most recommended to increase the hardness of cement is *terra puzzolana*: this is a volcanic product, consisting principally of clay hardened by fire and partially melted by a mixture of calx of iron. Its effects in the composition of cement are of little importance unless the cement be kept constantly wet. A cement made of powdered terras and lime was used by the Romans in all their aqueducts and buildings to contain or keep out water, and it has been generally employed in such structures since that time. When used in buildings which are constantly wet, it sets very quickly, and is then impenetrable to water. From this it has been rather hastily concluded, that it is the best substance that can be mixed in cements of all kinds. But cement containing terras does not become so hard, nor endure the weather so well as cement made of good lime and sand in proper proportions; on the contrary, it is very apt to crack and perish quickly in the open air. Its efficacy in water fences is only felt when it is kept always wet, and seems to depend on some properties which powdered terras have in common with all martial clays hardened and almost melted by fire. It absorbs water and promotes the

crystallization or fixing of the calcareous earth, and by this means swells so as to render the cement impenetrable to more water. That terras in cements operates in the manner just mentioned, seems to be proved, by observing that burnt clay, if it contain iron, and all martial ochres, which consist of iron and clay, have the same effect as terras in cement, and may be advantageously used instead of it. Water cement generally consists of lime and terras, but no sand, though this last seems to make the cement harder.

The substance which makes the best cement with lime, for the purpose of resisting water, is iron reduced to small particles, either in the form of iron filings, boring dust, or powdered scales of iron. These mixed with cement made of lime and sand in the proportion of  $\frac{1}{4}$  part or less, seemed to be of no service in dry situations; but in wet places, where cement made of lime and sand will not harden at all, they were very serviceable, and by the closeness of the texture produced by the rusting and swelling of the iron, excluded water perfectly. Mixed in a larger proportion with lime, such as five or six parts of iron filings to one of lime, they produce a cement, which will resist the steam of boiling water. A cement of this kind, which I saw lately, had resisted the force of the steam of boiling water for several months. Iron then, would probably, with lime in the composition of mortar, be a more effectual re-

medly for dampness in buildings than any substance hitherto used for that purpose. But in dry situations this does not answer, if the iron be more than one-fiftieth of the mass of mortar.

A late author has proposed to mix bone ashes with the cement made of lime and sand. One part of bone ashes to four of lime, he says, improves the cement, makes it dry quicker, and not so apt to crack, without injuring its hardness much, while it improves the tenacity. A quantity of bone ashes equal to that of the lime, is not so durable, but dries very fast, and is not injured by hot weather. More would diminish the cohesion of the cement, and less would be of no service. The bone ashes in cement seem to be useful by their being porous, in consequence of which they ~~admit air and moisture~~, and thus accelerate the setting of the cement, and secure the plaster from moist and variable weather, and prevent dampness.

As the strength and duration of our most useful and expensive buildings depend on the goodness of the cement with which they are constructed, an inquiry into the nature of cement is of great importance, particularly in this country, where the weather is so variable and trying, and the common cement so bad. In most of our buildings the timbers last longer than the walls, unless the mouldering cement be frequently replaced by pointing. It has been generally supposed, that the Romans were acquainted with a cement far superior to any now

### ON CEMENTS.

In their most exposed structures, the  
it has withstood even a trial of fifteen  
or two thousand years. But the nature of  
this cement shall be considered in another essay.

## ESSAY XI.

### *ON ANCIENT CEMENT.*

A. D. 1783.

Two kinds of cement have been found in the ruins of ancient buildings; one a bituminous, the other a calcareous cement. Bituminous cement consists of asphaltum or the bitumen judaicum: this substance is of a dark brown or black colour, of a soft or liquid consistence, found in great quantities on the surface of certain lakes, in different parts of the world, particularly in the Dead Sea. Some species of asphaltum are hard and solid, and the most fluid kinds become so by age. The thick and solid asphaltum is employed at present in Egypt, Arabia, and Persia, as pitch for ships; the more fluid species are used for burning in lamps. Vitruvius mentions, that Semiramis surrounded Babylon with a wall of brick cemented with a liquid bitumen, which was got in a very large lake in the neighbour-

hood, to which he gives the name of *Limne Asphaltidis*. He says, there are large lakes, which yield liquid bitumen also at Joppa in Syria, and in Arabia. Galen mentions, that in Judea there was a lake which was called *Asphaltites*, from the bitumen *asphaltum*, which was found in the Dead Sea. The *asphaltum* seems, in many respects, to be very well fitted for a cement in building. It is either fluid naturally, or may be made so by a small degree of heat, and it becomes exceedingly hard and tenacious by age. Those species which will not melt cannot be used for any purpose in building. But those which are fluid, or which melt easily, are very convenient; they soon acquire all the hardness of which they are capable, are not injured by being suddenly dried, but become equally firm and durable in the air, and impenetrable by water. These qualities of *asphaltum* render it a very useful cement, and though it is in some respects inferior to the calcareous cements, it answers much better for many purposes. But it is found in but few places in sufficient quantity to be employed in any large work. The Dead Sea furnishes the greatest abundance of it: it is found also in several parts of Europe, and a very little of it in Scotland. The district of Neufchatel is the only place in Europe that furnishes a considerable quantity of it. This species is of the consistence of honey, and is used as a cement for walls, pavements, water-courses, and for many other purposes. It has been found to answer there so well,



that it cannot be doubted that the asphaltum is a most excellent cement, well fitted for the purposes to which it was applied by the ancient inhabitants of the east.

But it was only employed to join bricks or stones with flat surfaces; its fluidity rendered it inconvenient when the stones were not fitted to each other. Perhaps, it would be a useful subject of inquiry to search after a substance, which might be substituted for the natural asphaltum in building. Common pit-coal seems most likely to answer this purpose. It furnishes by distillation an oil, and a substance like tar, which in its chemical properties, very exactly resembles the natural asphaltum, and like it is fluid at first, and becomes hard and firm by age. The substance like tar has been lately employed instead of pitch in the same manner as the natural asphaltum was used in Persia, and has been found superior to common pitch for preserving the bottom of ships. There is every reason to expect it would equal the natural asphaltum, as a cement in building. Perhaps, its price would prevent it from coming into general use now, in the same manner as the scarcity of natural asphaltum prevented it from being generally used by the ancients. The art of using asphaltum, as a cement, cannot be said to be lost. It was used formerly, only where it was found in plenty, and where that is the case, it is used still.

. This cement, however, is not in general superior

to the calcareous cement, and has some disadvantages, which that other is free from. It is very dangerous in case of fire, though it loses much of its inflammability and fusibility by age. It never acquires the hardness of the best calcareous cement, and is chiefly superior to it, with well-jointed brick and stone in thin walls, where it dries soon. It is not injured by moisture, which spoils the calcareous cement. All fusible and inflammable substances are liable to the same or greater objections.

Another cement was contrived, and much more generally used, in which lime was the cementing ingredient, and other substances were mixed with it, to remedy some inconveniences, which lime alone is subject to. A cement of this kind, has been used both by the ancients and moderns; but the ancient cement, it has been imagined by many, was superior to that of the moderns. It has been supposed, that the ancients mixed some ingredient in their cement, which is now unknown; that this long lost ingredient is the cause of the duration and hardness so much admired in ancient structures; and that the art of making this ancient cement is a lost art. But, as the moderns use a cement that very much resembles the ancient one, many have doubted whether this be really inferior to the ancient in any other respect than what is merely accidental. It has been imagined, that it is not probable that any knowledge once acquired in any art should be lost. That this is less probable, if the

art was a useful one, and practised by different people in different countries; that it is highly improbable, that the knowledge of a useful ingredient should be blotted out of the memory of so many people. Besides it has been asserted, that the ancients had no art in making cement, and that therefore it could not be lost; that the so much admired cement, which is to be met with in those ancient Roman aqueducts, and other structures, which have stood so many ages, ought to be considered as monuments of good luck, and not proofs of superior skill in the composition. This they endeavour to prove by shewing, that it is only upon very rare occasions, that the ancients were able to make lasting structures. When many fortunate circumstances concurred, the building was lasting; when these were wanting, it was not more durable than a modern house. They succeeded only when quarries of good lime, free of gypsum, were found near the building to be erected; when the limestone was very hard, and would not flake unless it was well burnt; when the building was at the public expence, and where there was no temptation to be parsimonious in the fuel; when the number of workmen employed was so great, that the lime was used as fast as it could be made, and when it ran no risk of being spoiled before using by long exposure to the air; and when the sand in the neighbourhood was clean, sharp, and well sized, resembling a mixture of coarse and fine sand. In such situations it has

been asserted, that it was not in the power of ignorance to make bad cement, and that chance furnished all that skill could desire.

The fashion of ancient buildings, it has been also said, gave their cement many advantages. The walls were of an immense thickness, intended to be supported by their own strength, and not held together by timber like modern buildings. This great thickness secured them from drying too fast; their great weight secured them from being shaken, and contributed to make the cohesion greater by bringing the different pieces, of which the wall was composed, into closer contact. The great thickness of the wall also took up much cement, and the quantity was greater than in modern buildings, because the walls consisted of small stones with mortar run between them. As the quantity of lime thus became immense, every method of saving it was of importance. Motives of economy led them to use as little lime and as much sand as possible; this, and not superior skill, led them to use the very best proportion of lime and sand in the composition of their cement.

From all these circumstances, it has been concluded that accident gave superiority to ancient cement, and put it in the power of any body to make it. Many other circumstances might be brought to prove the same thing, and that the ancients had no fixed rules to direct them in the composition of cement, and that its quality depended very much on accident. It might be said, that all old cement

is not good ; some of it is bad, and crumbles easily between the fingers. In the ruins of Herculaneum, and other ancient cities, much bad cement has been found. All ancient buildings have not lasted equally well, and we ought not to ascribe the duration of the few that remain to superior skill.

It may be said, that the preservation of the few, has been owing to accident, rather than superior skill ; that many circumstances had conspired to make the cement good, or that the building had been taken great care of, and the injuries of time prevented or repaired. Besides, though it be admitted that there was a secret in the composition of ancient cement, and that this secret has been lost, yet no body has been able to determine when this loss happened. The common practice of the admirers of the ancients, is to allow it to have been known in all the structures in which the cement is now found to be good, and to deny that it was known in those whose cement is of an inferior quality. Every body allows it to have been known even in the decline of the Roman empire. Most people also allow it to have been known to the Saracens. In many Saracen or Gothic buildings the cement is not inferior to that in the best Roman buildings, as is evident in many old castles and religious houses built since the conquest. If we admit that the hardness of the cement in the one case is owing to skill in making it, we must also admit it in the other. We may, indeed, suppose the supe-

and more of it would be necessary than of the present cement. The greater quantity of cement, and the greater labour in using it, seem in the opinion of the ancients to have been compensated by its hardness and durability. This then is one fixed principle in the composition of ancient cement, and probably one cause of its superior quality.

Another difference between ancient and modern cement, is that the modern is often made many weeks and even months before it is used. This injures the cement, prevents its ever acquiring a great degree of hardness, but makes it smoother and more plastic, and saves time in using it and mortar. This seems to be the great principle, to which every thing in modern buildings is sacrificed. In ancient walls, consisting of small stones run together with mortar, this could not have been used at all. The ancients, as we are informed by Vitruvius, knew this method of souring mortar; it was used only for the Opera Albaria, or in the inside of buildings; but the proportion of lime is too great in it ever to acquire the hardness of ancient cement.

Both good and bad cement are to be met with in ancient buildings, but this is not a proof that the ancients had no fixed principles in its composition. It might be owing to several causes. Good lime and sharp sand are not every where to be met with, and even when they were, they might not be used in the proper proportion from the carelessness of workmen. Even when none of these causes oper-



was good only by accident. It, on the contrary, is very uniform in its structure, and differs regularly in its appearance from the modern. This constant difference cannot be owing to chance. It is more probable, that this difference in the composition, is the reason why the ancient cement is superior to the modern. It seems to be but reasonable to allow, that the ancients had discovered that this composition gave to their cement greater hardness and durability. And this becomes more probable, when we find from Vitruvius, that the ancients had a cement like ours, which they used only for ornament or in the inside of houses. This is what they called the opus arenatum, but it is not superior to modern cements. The species of cement, which they employed in building their Cæmentum, was uniformly different from our mortar. It differs in containing less lime with a mixture of coarse and fine sand. When sand consisting of a proper assortment of coarse and fine particles could not be got, they substituted coarse powdered brick for it, and produced a very good cement: the signinum, according to Pliny. This mixture of coarse particles gave greater strength and cohesion to the mortar, made it more difficult to break in any one direction, and better calculated to resist the weather. This contrivance also enabled them to mix more sand with lime without injuring its plasticity. The mixture of coarse and fine sand is not used by the moderns, because it would be more difficult to manage,

town another cause may have spoiled the cement: it was buried in lava, the heat of which may have injured it.

The badness of materials, and carelessness on some occasions in mixing and using them, and neglect of securing the building for a sufficient time, seem to be the causes why in some ancient public buildings the cement is of a bad quality. This is no proof of the ancients having no fixed principles, and it is improper to say that their good cement was a monument of good luck, and not superior skill, since it has been shewn that their cement differs uniformly from that of the moderns in many respects. That using a smaller proportion of lime cannot be supposed to have arisen solely from motives of parsimony, otherwise it might have been expected in public buildings that more lime would have been used, and the cement of a worse quality. Yet the same persons who assert that parsimony made them use little lime, assert also that in public buildings where there was no temptation to economy, more fuel was used, and the superiority of the cement was owing to the lime being better burnt.

Vitruvius also most accurately points out the qualities as well as the proportion of the ingredients. He observes that the sand should be such that it makes a noise when squeezed in the hand, and that it should not soil a clean cloth upon which it is thrown, and he recommends to mix coarse powdered bricks with the cement when the sand is of a bad

quality. He also differs very much from the moderns in preferring pit sand to river sand, and says that when none but river sand can be got, that it should be mixed with one-half its weight of powdered bricks. He mentions that the hardest limestone makes the best lime for building, and that the more spongy was better for ornament. In a chapter *de maceratione calcis ad opera albaria et testoria perficienda*, and in another where he treats of the *opus arenatum*, he mentions every thing that is in common practice at present, such as keeping the plaster six months before it is used, otherwise it will crack; mixing hair with it, using two or three coats of the *arenatum*, or sand cement, covering all with a coat of the *opus albarium*, which was made altogether of lime, making scratches on each coat to help the next coat to adhere to it. But, he adds, these will only serve for ornament, not for building. The modern practice is always to keep the cement some time before it is used, and to this is owing its inferiority. The ancients knew this as well as we, but they confined it to its proper sphere for ornament in the inside of houses. He mentions also when the cement was intended to be of the strongest kind in aqueducts, that pieces of flint should be mixed with the cement, and that none of them should be above a pound weight. This we see was practised in the aqueducts, whose cement is now so much admired. The art of making ancient cement cannot be said to be lost, since we have the proportions and manner of using the cement

in Vitruvius, and may see their effect in old buildings, in which, by chemical experiments, we can discover the exact proportions he recommends. But though the secret of ancient cement is not lost, the art of making it is not practised in this country.

It is difficult to find out a reason why ancient cement was laid aside, and one so far inferior substituted for it; or when this change took place. Perhaps it might be owing to stone buildings cemented, with lime being much less common at the time this change happened than at present. Formerly well cemented stone buildings were either public edifices, or the seats of the chief nobility. They were not only intended for lodging, but were places of defence. No expence was spared to make them as strong as possible. Their walls were thick and massy; they required an age to build. The walls were cementitious, in the manner of the ancient Roman buildings, and their cement has every property of the ancient cement. But though persons of higher rank were lodged in houses whose cement was not inferior to the old Roman, the middling and lower orders were lodged in very mean habitations. Their houses of stone were not so well built as the walls that inclose our fields, and the wind and rain were imperfectly kept out by a perishable cement of clay and sand. The greatest part of their houses were of turf, and a very few of wood. While property was not secure, and when the common people had little liberty, they never thought of, and were

incapable of affording more durable and expensive houses. The demand for substantial stone structures was so small, that two or three master-builders were sufficient for a whole kingdom. When liberty increased, and property was more secure, riches accumulated among the lower ranks ; they then began to wish for better lodging than their miserable huts afforded. The building a house became a mark of riches and successful industry, and helped to transmit the name of the builder to his posterity. This species of property was then better suited to the circumstances of ordinary people, than property in land, and could be more easily obtained. They wished for something more substantial and less dangerous than wood, and cheaper than the massy and dismal dwellings of the great. A species of building suited to their circumstances was contrived, and the expence was diminished by raising thinner walls, and making the mortar in great quantities at once, and of a smoother consistence, exactly like the *opus arenatum* of the Romans. By this means less mortar served, and labour was saved. The demand for these slighter buildings becoming great, the trade fell into more hands, and instead of two or three builders in a kingdom, half a dozen could be met with in every town. Every one strove to make houses cheaper and slighter than his neighbour, without injuring their appearance, and houses could be built of any dimensions, at any price, and to last any number of years desired. This slight species

of building became so general, that the ancient masonry walls built with a great quantity of strong cement were laid aside, and are so little in request at present that very few workmen know any thing about the manner of constructing them. It is, indeed, scarce possible to find one who will alter the mode of building to which he has been bred, even when a masonry, durable, and expensive public edifice is to be erected.



## ESSAY XII.

### ON THE DIAMOND.

**T**HE diamond is the hardest of all minerals. Its specific gravity is nearly 3.5. When brought into Europe in its rough state, it is either in the form of round pebbles, or in crystals of an octohedral form. Sometimes it is met with like hexagonal prisms terminated by eight-sided pyramids, or in regular cubical crystals with the angles truncated, or cut off. Whether these differences be original or adventitious, has not been determined. Those in the form of hexagonal prisms, are commonly reckoned the hardest, and may therefore have preserved themselves against accidents better than those that are rounded, which are said to be the least hard; and of consequence, more liable to be altered by friction upon other substances. The round diamonds may be supposed to have undergone the same changes with many of

*the common* rock crystals that have been torn from their beds by changes in the earth, and by agitation with water ground and rubbed against one another, till they have been reduced to a round figure.

It is not known whether there is any rock where the diamonds are found, which has the properties of these precious stones, or, which has served as a basis or matrix for their formation, in the same manner as quartz is the basis of rock crystal. The account which travellers have given us of the natural situation of diamonds in Golconda and Visapore is, that they are found separate at a considerable distance from each other, but no mention is made of the quality of the rock in the neighbourhood of the diamond mines. When the miners meet with rock before they can get to the stratum of earth where the diamonds lie, it would appear from their manner of working, that there is nothing remarkable in its qualities. The properties of the diamond are so singular, that had the rock in the neighbourhood possessed any of its qualities, a part of it would before now have been sent into Europe, or examined and described by the travellers who have visited these mines.

Diamonds are found in Golconda and Visapore in the island of Borneo, and lately in the Brazil. The continent of India, both on this side and beyond the Ganges, is remarkable for its mines. The diamond mines are generally near rocky hills and mountains. A chain of these begins near Cape

Comorin, extends in breadth near fifty miles, and runs quite through Bengal. In, among, and near these hills there are many mines known; but the barrenness of the country, the want of water, the badness of the roads, and the savage state of the original inhabitants, who still possess this part of the country, and who have little commerce with their neighbours, together with the tyranny of their petty princes have prevented these mines from being properly examined, so that a great part of them is concealed or wrought with great secrecy, lest their produce should fall into the hands of their rapacious rulers. But the kingdoms of Golconda and Visapore have a sufficient quantity of diamond mines to furnish all the world plentifully with these gems. Their kings however, permit digging only in a few parts, lest, as it is said, the diamonds should become too common, and in consequence of this, diminish in their value.

All stones above a certain size are their king's property, so that few of extraordinary magnitude are brought to Europe. Diamonds do not lie in clusters; they are found in detached pieces at a greater or less distance, according to their size. Those mines which yield large stones, frequently do not contain above one in a quarter of an acre; those that yield small stones may contain a great number in the same space. There are about twenty-three diamond mines in Golconda and fifteen in Visapore. In these, the diamonds are commonly

found in a stratum of reddish earth, in many places, not unlike the gravel pits in Britain, which stains the miners' clothes, and appears not to differ much in its qualities from the red clays, or common iron ore of this country. One mine in Golconda and one in Visapore have this stratum of a yellowish earth, and one in Golconda of a blackish earth. The depth of these mines is commonly three or four fathoms. They seldom can dig deeper on account of the water, which they have no method of removing. In one of the Golconda mines, they are obliged to dig through rock five or six feet deep before they come to the stratum of earth in which the diamonds lie. This mine is between forty and fifty fathom deep. They work through this rock with great difficulty, as they seem to be altogether unacquainted with the use of gunpowder in mining. They, however, in some measure, supply its place by lighting a large fire on that part of the rock through which they intend to pass: when the heat has penetrated the rock to a sufficient depth, they split it by throwing cold water upon it. In one place, the diamonds are found in a small vein of reddish earth, about half an inch or an inch in breadth, running irregularly through the rock. This earth, upon being pulled out, is found to contain diamonds. In the kingdom of Bengal, and in the island of Borneo, diamonds are found mixed with the sand of rivers. Some of the stratum of earth in which these stones lie, is washed down from the sides of hills by the rains, which fall in great quan-

tity at a particular season of the year. Those who search these rivers for diamonds, prefer the parts where the heavier kinds of earth, particularly the pyrites have been deposited. They begin their operations in the end of January or beginning of February, immediately after the rains are over.

The earthy matter in which the diamonds are found, covers them so perfectly, that it is with the utmost difficulty that they can be distinguished from an indurated iron ore. It adheres so strongly to them, upon many occasions, that it cannot be got off, but by grinding or rubbing them against one another. From this, as well as from the foreign matter that is frequently found in the centre of diamonds, it would appear, that they were once in a soft or glutinous state, or that it was not till long after their first formation, that they acquired that hardness, for which they are so much valued. In consequence of the diamond, when bedded in the earth, having so little of the lustre by which it is so easily distinguished in its polished state, it is necessary to examine the earth in which they lie with the utmost attention. In order to render this as easy as possible, the lighter earths and those parts that approach in quality to common mould are removed by agitating the whole with water, and pouring off immediately the muddy fluid. As the diamond is among the heaviest of the earthy substances, there is little danger of its being carried off in this manner.

Those parts which are not carried off by the water, are dried in the sun, and carried to a square place inclosed by a high wall, and carefully examined. Every thing that has the smallest resemblance to the diamond, is rubbed against the common kinds of stone. If it does not discover itself to the satisfaction of the miners by this method, they strike it with violence against a hard body, and sometimes with the iron instrument with which they work. By this imprudent method, many of the finest diamonds have such cracks or flaws formed in them, that their value is greatly diminished.

The value of the diamond is ascertained by weights called carats, each of which is divided into four grains, and these grains again into fractional parts. The smallest diamonds, in the state in which they are dug up, are seldom less than  $\frac{1}{4}$  of a carat or  $\frac{1}{8}$  of a grain. The largest diamond known, is a rough one, in the possession of the Portuguese crown, which weighs no less than 1680 carats, or eleven ounces twenty-eight grains, nearly. There are several other diamonds of extraordinary size in Europe, but not at all approaching to this. Rough diamonds are valued at 2l. for the first carat, by the best authors, and afterwards their value is computed by multiplying the square of the number, expressing their weight in carats by 2, and the product is the value in pounds sterling. But this price is obviously too high for large diamonds, for which no purchasers would ever be found at these extravagant rates. The



lustre, the transparency, and the figure of the diamond also affect its value very much.

The diamond, as it comes out of the mine, is covered with a dark obscure rust, which scarcely shews any thing of the brilliancy for which the stone is valued. In this state the best connoisseurs cannot judge of their value. The crust cannot be removed by rubbing with the hardest substances. No other method will answer but rubbing one diamond against another. The small particles that are thus torn off are carefully collected, and used for the cutting and polishing of other diamonds, as well as for cutting all other stones. When the external crust, which appears to be in a different state from the internal parts, is removed, the quality of the stone can be determined. Many that before this appeared to be good, are found to be full of cracks or flaws, which either were originally formed in them, or had been produced by the rude treatment which they frequently meet with from the miners. By removing the superficial crust, many imperfections are perceived which could not be observed in their rough state: such as black specks, which are frequently formed in their centre, and which seem to be of the nature of the diamond, but in a less perfect state; and sometimes materials totally different from the diamond are surrounded by a crust of that substance.

The colour of the diamond varies prodigiously. We find them, from being perfectly colourless and transparent, of various shades of red, yellow, green,

and even black. These colours, however, are never of a very deep tinge. We never meet with them of a deep red, yellow, or green. They only have a faint tinge of these colours. The diamond that is perfectly transparent like the rock crystal, is more valued than any of the rest, and next to them, those that have a faint green tinge, but quite transparent, are preferred. Those of a reddish or brownish colour, or opaque, are the worst. It is commonly very difficult to discover the true tinge of this stone, especially if it has been cut in the brilliant fashion, that is, if it has been cut to a number of points by a number of planes variously inclined to each other. For it possesses so great a refractive power, that if the reflection of the light, which would have passed through it, be prevented by a black foil placed below it, so that no light comes to the eye, but what is reflected from its surface, this light passing through the planes variously inclined to one another, and which act as so many prisms, is, by the high dispersing as well as high refractive power of the diamond, separated into as many spectrums as there are planes; and these, if there is the least motion in the stone or undulation in the light so dazzle the eye, that it is impossible to distinguish the light which the diamond is disposed to reflect from that which is separated into the colours by the diamond itself in consequence of its prismatic figure.

The diamond is of a laminated texture. Of this the diamond-cutters are extremely sensible, as they

find it much easier to split them in one direction than in another. This direction they are obliged to attend to in the polishing. It is easy to polish diamonds in the direction of their plates or fibres ; it is impossible to polish them in a direction different from this. The same thing is evident from the manner in which diamonds cut glass. It is only in the direction of their plates that they do this. If the diamond be drawn along the glass contrary to this, it scratches the glass, but does not split it.

It has been imagined, that the diamond does not differ in the nature of the earth of which it is composed from rock crystal : that it owes its great hardness to the heat of the climate in which it is found, and to its being removed from the rock which served for its basis. This has been imagined, from its being observed that rock crystals which have never been adherent to the rock, but which have been crystallized in their own bed or matrix, are always the clearest, and next to them, those which, by some unknown accident in nature, have been separated from their basis. Whereas, those that adhere to the rock are always impure and seldom good for any thing. But if the qualities of diamonds were different from those of rock crystals in consequence only of being removed to a greater distance from the matrix that served for their formation and crystallized in a warmer climate, we should surely meet with rock crystals in states of hardness and lustre inter-

mediate between them in their ordinary form, and the diamonds according to the climate in which they are found. But the rock crystal which is found on the coast of Barbary is equally inferior to the diamond in all respects, that the crystals in Lapland are. Others, with more probability, have imagined that all the other commonly reputed precious stones, such as the ruby, the sapphire, the topaz, and the emerald, were diamonds in an imperfect state, or which failed in being diamonds by the mixture of some metallic substance. In consequence of this, it has been asserted, that could we separate the colouring matter from these stones, they would not be distinguished from the diamond. On this account, some of the best naturalists, even Cronstadt himself, call the ruby the *adamas ruber*. It differs but little from the diamond in hardness and weight. Others call the sapphire the blue diamond. It is even pretended, that the diamond is found in a soft state, as if it had not remained long enough in the earth, since its first formation, to acquire the hardness it is capable of acquiring. To this substance, the English jewellers have given the name of jargon. The natural shape of this substance is not known. It is said that it is found in the East Indies, in the form of pebbles; that it is there split into pieces and sent into Europe; that it is of different colours, white, light yellow, and brown. According to some lapidaries, it comes nearest to the sapphire in hardness.

As these stones resemble some diamonds in colour, they are called soft diamonds; and it is said, that many have been imposed upon, and purchased them for true diamonds. They, however, have none of that lustre which depends on the high refractive power of the diamond, and resemble more the topaz in this respect.

The colouring principle of many of the precious stones, and the colour of the diamond on some occasions, has been commonly believed to be metallic: but this is far from being universally the case. We know indeed, that glass by the proper mixture of metallic calces, may be made to imitate the colour of all of them pretty exactly. But, it is at the same time well known, that the colour of the artificial stones is, for the most part, of a much more fixed kind than that of the native gems, and that they sustain a degree of heat without injury, which entirely dissipates the colouring principle of most of the gems. The ruby, however, which approaches so nearly to the diamond in hardness and specific gravity, and which agrees exactly in its figure, cannot be deprived entirely of its colour in the most violent degrees of heat. While the sapphire and the other coloured precious stones lose their colours in a very gentle heat.

Reasoning from analogy, joined to an universal disposition in mankind to appear more wise than they really are, has greatly retarded the progress of natural knowledge. It is in this way, that some

and some of the principles themselves are capable of being decomposed. Unless from the general estimation in which the diamond is held, it is difficult to find why it was imagined to be the pure elementary earth.

I think it can be demonstrated, as well as any thing can be demonstrated by experiment, that it is far from being a pure earthy body. It is observed of the diamond, that it acquires the property of shining in the dark, in consequence of its being exposed to the sun's rays. This property most of them possess, though it is observed by Du Fay, that one very fine diamond had not this property in the least degree. The light which the diamond emits is very faint and of short duration: it cannot be perceived unless the person who is observing it has remained in a dark room, till the pupil is dilated as much as possible. This property of the diamond, as well as its attracting light bodies, is generally believed to be of the electrical kind. It has been long known, that the diamond is incapable of fusion. It has been exposed to the most violent degrees of heat in furnaces, and by the Grand Duke of Tuscany to the focus of Tschirnhausen's speculum, both by itself and in mixture with alkaline salts, flints, metallic substances, and ashes. It was constantly found, that whatever other alterations it might undergo, it never shewed the smallest disposition to fusion; and what was more remarkable, it was always more altered in its qualities when exposed alone than when mixed



with other substances. The ruby exposed to the focus of the same mirror for forty-five minutes, became in a little time skinning, as if it had been rubbed with melted grease: air bubbles formed upon its surface; it lost considerable part of its colour; its angles became blunt; and it acquired such a degree of softness, that it received the impression of a seal, and was easily cut or indented with a knife. In these experiments, however, as well as in those made by order of the Emperor Francis the First, in violent heats raised by means of fuel, it was found, that of all the precious stones, the diamond was most affected by the heat. If they were coloured, when exposed to the focus of the speculum, they soon became colourless, lost their brilliancy, and became opaque; air bubbles formed on their surface, and they lost considerable part of their weight, and at last were entirely dissipated. While the ruby in the same degree of heat lost nothing of its weight, nor changed the least in its qualities, the sapphire, though it lost all or the greatest part of its colour, lost none of its weight; and the emerald, though it melted, lost little of its colour, and none of its weight. One would have imagined that some attention would have been paid to these experiments, in the arrangement of the precious stones, as they clearly prove that they differ considerably from one another. But none seems ever to have been paid to them, till the inquiry into the nature of the precious stones was lately revived by some of the French chemists. The

Chevalier D'Arcet was the first who repeated the experiments of Francis the First, in a furnace for baking porcelaine, and he found that the diamond really evaporated. He observed, that this happened in a very moderate heat ; that there was no occasion, in order to discover this, to expose it to the focus of mirrors or lenses, nor even to such violent degrees of heat as were raised in the furnaces of Francis the First : that all that was necessary for this purpose, was to heat them red hot. He satisfied himself of this, by exposing them first to heat in open vessels, and he afterwards varied the experiment by surrounding the diamond with porcelaine paste, and closing all the openings with the same paste diluted with water. The porcelaine balls including the diamond were put into the porcelaine furnace, and exposed to the degree of heat necessary to bake the porcelaine. In some of the balls the diamonds remained unaltered. In others, they had lost more than half their weight ; in others, they were entirely dissipated, without the smallest mark or flaw being perceived in the porcelaine crust. These differences, M. D'Arcet was disposed to account for, from the different nature of the diamonds made use of in the experiments ; to some of them being Brazil diamonds and other East Indian ones. From these experiments, it was concluded, that the diamond might be distilled, and by this means, a number of small diamonds put into a retort and distilled, might be united so as to produce a large one. Lapidaries and jewellers insist, that

diamonds may be exposed in close vessels to heat without losing any thing of their weight. When they meet with brown or blackish diamonds, they frequently alter their colour, and render them paler by exposing them to a considerable degree of heat. By these means they diminish their imperfections and increase their value. Macquer and some other chymists have repeated these experiments of D'Arcet on the diamond: they all agree, that in vessels that are perfectly close, nothing distils from the diamond, nor are its qualities altered in this situation in the least. It has been put into an earthen retort with a receiver luted to it, and exposed to the most violent degrees of heat that can be raised by means of fuel, without alteration. It has been surrounded with charcoal dust and exposed in a covered crucible to the most intense heat without losing either its polish or its transparency, without losing any thing of its weight or producing the least alteration in the charcoal in contact with it, or shewing the least appearance of fusion. When exposed to heat in contact with chalk and calcined hartshorn, it loses a little of its weight and sometimes its transparency, so as to resemble a rough diamond. The result of these experiments is different from those of D'Arcet, the cause of which is, that in his experiments, the diamond was put into porcelaine paste, which requires a great heat to bake it into china. For a long time it is in a spongy state; during this time the external air might come in contact with the diamond,

and contribute to its dissipation long before the heat had become so great as to render the porcelaine crust perfectly firm and solid.

I made an experiment lately on the effect of heat on the diamond. The degree of heat to which it was exposed was about as great as is necessary to cupel silver. The diamond was put into a cupel in an assay furnace. It was about  $\frac{3}{8}$  of an inch long, and  $\frac{1}{4}$  of an inch broad. It had remained but a short time in this situation, when a part in the middle of the diamond became apparently more prominent than the rest. The diamond was manifestly more luminous than the cupel or muffle that contained it, and a gentle undulating phosphorine flame was perceived on it, which was greater when a greater quantity of air was admitted into the muffle. After an hour's exposure in this manner, it was taken out and examined. It was reduced to about one half of its former size, had become white, and lost its transparency, and seemed as if the plates of which it is composed, had been separated from each other and resembled a rough diamond. It was returned to the fire, and kept above three hours longer in a less heat, during which, it gradually diminished in size, till it was not above the size of the head of the smallest pin. Probably, had the heat been a little greater, or continued some time longer, it might have been entirely dissipated. A ruby nearly of the size of the diamond was exposed

along with it, but was not altered in the least. Mr. Macquer and M. Mitouard agree in having perceived, that it manifestly emitted flame when exposed to heat in the open air and dissipated entirely. The French chemists exposed a great variety of other precious stones in the same circumstances with the diamond. These sometimes lost their colour, became soft, or even melted, but none of them lost any thing of their weight. These experiments point out a manifest difference between the diamond and the other earthy substances, and shew that it resembles the inflammable or metallic bodies more than the earths.

The diamond is inflammable: it burns in the open air, without leaving any ashes, or emitting any smoke that can be perceived. It seems to be among the purest of the inflammable substances. With these it agrees in its great refractive power, and in suffering no alteration by being exposed to heat, provided the air be excluded. It is not a volatile body, but is dissipated merely in consequence of its inflammation, in the same manner as charcoal, and the more fixed inflammables are. It is difficult without more experiments to say to what class of the inflammables it belongs, or whether it be a mean betwixt an earth and them. It resembles the earths in hardness, but it differs from them in producing flame by heat, and dissipating as it burns; while the flame which it produces is small, brilliant, weaker than that

emitted by any inflammable when burning, and resembling more the luminous appearance of the phosphorus in the ordinary heat of the air, or the light emitted by tin or bismuth when heated a little above their melting point.

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#### NOTE BY THE EDITOR.

To the information contained in this account of the natural and chemical properties of the diamond the experiments and observations of thirty succeeding years have made a few additions, which are altogether corroborative of the opinion of the nature of that curious mineral, delivered in the latter part of this essay. It is now clearly ascertained, that the diamond is of a carbonaceous nature. Lavoisier, by means of a large burning lens, consumed four grains and a half of a diamond under a receiver containing 60 cubic inches of air. During the process, the air suffered a diminution of nearly  $\frac{1}{4}$ , and the remainder precipitated the lime from lime water; that is to say, carbonic acid was formed by the union of the diamond and oxygen gas. But charcoal forms the very same product with oxygenous gas, and therefore the diamond and charcoal are either the same substance, or contain the same substance. It is ascertained, however, that diamond yields during combustion a larger quantity of carbonic acid than charcoal. From which it has been concluded that diamond is pure carbonaceous matter, and that charcoal is its oxyde. This is in all probability just, but still not perfectly demonstrated. It is clear that diamond contains more carbon than charcoal, but it is difficult to see any satisfactory evidence that charcoal is oxyde of diamond. At least it is known that charcoal



this. In many instances it is coloured by a volatile matter; there are frequently black specks contained in the substance of the diamond; and the outside of the crystal is always covered in its natural state by a coat of extreme hardness, and no lustre, probably of the nature of the diamond itself. We have now a clue which may possibly lead us to the determination of the cause of these different circumstances.

The colour of diamonds is of various tinges; in one respect, however, these tinges frequently agree, in volatility. Many of them, when the diamond is exposed to heat in close vessels, disappear. This property does not correspond with the supposition of a metallic colouring principle; at least, the metals usually found to give colour are of a more fixed nature. Besides the metals do not impart any colour till they are oxydated, and it is difficult to imagine how an oxyde of any kind could exist in the substance of the diamond, which attracts oxygen with so much greater force than any metal. The most probable source of this colour appears to me to be carbon. It is difficult to guess how the diamond has been formed by nature, or through what changes or situations it may have passed. But whether it has been always diamond, or formerly only carbon, it may be conceived to contain a little oxygen united with some of its particles, which the application of caloric may cause to fly off in the state of carbonaceous oxyd. It is not easy to drive all the colour out by heat; the last portions probable are less oxydated and consequently less volatile. The truth of this hypothesis might be determined by repeating Lavoisier's experiment quoted above, but substituting a very dark coloured diamond in place of a perfect one. If the colouring matter be really an oxyde of diamond, less carbonic acid will be produced by the same quantity of base.

The specks found in the substance of diamond are of a black appearance. I do not doubt that they too are oxyde of diamond or charcoal, proofs of partial decomposition or imperfect formation, and of consequence chiefly in the consideration of the probable origin and ancient natural history of this mineral. The crust

of the rough diamond is also likely to turn out carbonaceous. It is well worth examination, the carbon would perhaps be found united to a portion of the neighbouring stratum. The experiments already made on diamonds have shewn that an appearance resembling the crust of the rough diamond may be produced on the polished mineral by exposing it to heat in contact with chalk.

After the consideration of the facts now discovered regarding the diamond, the mind naturally turns itself to the possibility of forming diamonds artificially. It cannot be said that this discovery, if made, would be of any great advantage to mankind. It is difficult to conceive to what use this substance could be generally applied, and though the invention of a method of making diamonds might possibly procure a fortune to the inventor, a publication of the process would infallibly lessen the value of diamonds so prodigiously that the manufacture would scarce be worth attempting. If our theories be right, however, there is no want of materials from which diamonds might be made. All nature abounds with charcoal or its combinations, and whole mountains, nay kingdoms, are composed of chalk and limestone, in which diamonds ought to be contained in astonishing abundance. But plentiful as this principle may be, there appears at present no likely method of obtaining it pure. The combination of the oxide of diamond with oxygen itself cannot be decomposed by art, though nature daily effects it in the vegetable economy, nor these immense quantities of charcoal set free which would contribute so materially to the accommodation of the human race. Much less then are chemists able to decompose charcoal itself, to which the first dose of oxygen must adhere with greater force than any succeeding one. Carbon, of all known substances, attracts oxygen most strongly. No single affinity therefore can dissolve the union of diamond and oxygen. But better hopes may be entertained of double affinities. In this way a sudden decomposition of charcoal could only produce some very small grains of diamond in the form of fine powder. To make a

crystal of that substance would require either a process advancing very slowly, and allowing the particles time to arrange themselves regularly, or some means of reducing the whole to fusion. Phosphorus and charcoal, and sulphur and charcoal distilled together, do not leave a residue having any resemblance to the diamond. This seems a contradiction to Berthollet's doctrine that a large quantity of a weaker body will always partially decompose a union supported by stronger affinities. On the whole the process of making diamonds does not seem at present very likely to be put into practice, and it must be confessed that the whole proposal bears a very romantic aspect, and is apt to remind one of the times of alchymy.

The natural history of this curious mineral is no less extraordinary and interesting than the account of its chemical properties. To geologists it ought to afford the most important considerations, and it is much to be wished that some well-informed mineralogist should view and report the state of the strata or rocks in the vicinity of which it is found. The nature of the stratum in which the diamond itself lies it is particularly desirable to know. It appears that the diamonds do not lie in clusters, at least that many are not found together. They rather seem to be thinly sown over a large surface. It is pretty clear, that they have been fluid themselves, or precipitated from a fluid. Their crystalline form shews this, as well as the extraneous substances which they occasionally invest. It is altogether impossible to decide whether they have always been in their present state as to chemical properties or not. But it seems a more probable supposition that they have been formed from common carbonaceous matter, during some of the revolutions and processes which have taken place in the great laboratory of nature.

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## ESSAY XIII.\*

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### *ON THE QUANTITY OF MATTER IN BODIES.*

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**T**HAT the quantity of matter in bodies is in proportion to their weight has long been a received opinion. The substances, which we are commonly acquainted with, are so manifestly possessed of weight, that it never was doubted that all bodies were possessed of it. Among these bodies some, though of equal bulk, are heavier than others ; a circumstance not difficult to account for, by the particles of matter in the one body being more closely pressed together than they are in the others. As all bodies, when raised to a small height above the surface of the earth, upon being deprived of support, immediately fall towards it, it was difficult to imagine any substances deprived of this property : and though smoke and vapour rise upwards, instead of falling downwards, it required but a small share of philosophy to see that this

\* This essay is dated February 12, 1773.

depended upon these bodies being lighter, bulk for bulk, than the air, which every where surrounds this globe ; and that the tendency of smoke and vapour upwards, was of the same nature with the rising of a piece of cork or wood to the surface after it had been pressed to the bottom of a vessel containing water.

That the quantity of matter is in proportion to the weight of a body, is so agreeable to the common sense of mankind, that it seems scarce ever to have been called in question. When natural philosophy first began to be cultivated to advantage, by fact and experiment, instead of fancy and hypothesis, stronger proofs of this doctrine were soon obtained. It was found that all bodies, how different soever in other respects they might be, upon being let fall from the same height in vessels where there was no resistance from the air, apparently fell through equal spaces in equal times ; and that therefore gravity acted upon all bodies in proportion to their quantity of matter, otherwise this could never have happened. The chemists, in the experiments by which they attempted to investigate the properties which distinguish bodies from each other, though they frequently found that something had escaped them during their operations, and which might easily be accounted for from the volatility of some part of the body, were often surprised to find, that the weight of the principles into which the body apparently was resolved, instead of being less, was greater than it

was before : and this happened in situations where it was difficult to understand upon what this increase of weight depended. This is in no case more, remarkable than in various experiments that have been made on metallic substances. These bodies, it is well known, upon being exposed to a proper degree of heat, lose almost all their distinguishing properties ; from a hard solid substance they are converted into a heavy powder, which, except in specific gravity, has scarce any resemblance to the body from which it was produced. What has the most attracted the attention of the chemists in these experiments, is the increase of weight, which the metallic substance acquires by this process : an increase which, though different in different metals, is, however, considerable in all of them, that are capable of being calcined. This increase, on many occasions, is one-tenth, in some it is one-eighth or more. In this process the metal does not seem to be acquiring any thing from the air, the only body with which it is in contact. On the contrary, it seems rather to be losing something, as appears probable from the alteration in its qualities, and the smoke which constantly arises from it during this process. The cause of the increased weight in these experiments has generally been supposed to be a quantity of igneous particles, which adhere to the metal after its calcination. If this has any meaning at all, it must be, that a quantity of the heat of the fire adheres to the metal, and increases its weight. But whatever additional heat



was separating, that the weight increased while the quantity of matter diminished.

Dr. Black, upon considering these experiments, hazarded an explanation, that involve consequences which were little imagined when it was first proposed. All the chemists agree that there is a principle common to inflammable and metallic bodies. Dr. Black imagined that this principle is possessed of this singular property, that it not only is not subject to the power of gravity, but is possessed of one directly opposite to it; so that instead of increasing the weight of the body to which it is united, it remarkably diminishes it. As this was exactly contrary to the common ideas of mankind concerning matter, the apparent absurdity of it was attempted to be taken off, by endeavouring to shew that the idea did not involve an absurdity; that it is easy to conceive a body with a repulsive power towards the earth, and that this is of the same nature with the pole of one magnet repelling that of another; and that besides, whatever is the cause of gravity, is not subject to its own effect.

Inflammable substances, according to this doctrine, should be heavier after their inflammation, or after the separation of the principle on which this depends, than they were before: this is proved by observing, that phosphorus of urine, exactly counterpoised in a balance, upon being set on fire, burns with great violence, the scale on which it is preponderates, and it gains near three drachms and a half in the

ounce, or almost one half. Those persons too, who employ themselves in burning sulphur, in order to make the vitriolic acid for the purposes of bleaching or dyeing, obtain, independent of the water mixed with the acid, at least twenty ounces from every pound of sulphur, though a suffocating vapour is felt every where in the room, while the sulphur is burning. Some of the metals, particularly zinc, burn with as much violence as any inflammable substance, and with an evident smoke. By this process a calx remains heavier by one-sixth than it was before, which dissolves in acids, and forms with them the same compounds as formerly, with no alteration in weight, and with no effervescence. Acids, therefore, which effervesce with zinc in its natural state, separate from it the same principle which fire separates; and the separation of this principle is the cause of the effervescence, and the cause too why the vapour which arises during the solution is inflammable. If a piece of silver be put into aquafortis, contained in a vessel so constructed, by means of a valve, that any thing may pass out of the vessel but nothing can get into it, I find, that as the silver dissolves in the aquafortis, the scale in which they are preponderates; though, during the solution, an elastic vapour came out of the vessel, which, by its smell, could be perceived every where in the room in which the experiment was performed. The increase of weight here was not so small as to be ascribed to any fault in the balance, for it was above

thirty grains in the ounce of silver; and nothing could have got into the vessel but what must have passed through the pores of the glass.

When we take the calces of metals, either prepared by fire, or precipitated from their solution in acids by caustic alkalies, and expose them to the sun's rays, we find that they recover the properties they had lost, and dissolve in acids with the same appearances as at first. When we enclose them in glass bubbles that are hermetically sealed, we find that they become lighter than they were before.— This is related by Mr. Boyle to have happened in many experiments made, with a view which he does not mention; but who surely cannot be supposed to have been misled by any partiality for an opinion concerning the quantity of matter in bodies which he did not entertain. If the heat of the fire, or the air in contact with the metal during its calcination, be the cause of this increase of weight, why does a similar increase appear by solution in acids, or by precipitation with alkali, where there is no increase of heat and no air? Or, if it be part of the acid, the water, or the alkali, that is the cause of this, why should the same thing happen in the fire where there is neither acid, alkali, nor water near it? Or why, in hermetically sealed glass vessels, should the rays of the sun restore the metal to its original state, and diminish the weight of the body? \* The rays of

\* It can scarcely be necessary to remark, that the more recent discoveries in chemistry afford a satisfactory answer to these questions upon different grounds.

light, or something equally subtle with the rays of light only could get at the body in this situation. That the separation of a principle from bodies increases their weight, and the addition of it diminishes it, and that weight is not the measure of the quantity of matter in bodies, seems to be as well established by experiment, as experiment is capable of establishing any thing. At the same time, if weight be not the measure of the quantity of matter in bodies, or what is the same thing, if gravity does not act upon bodies in proportion to their quantity of matter, some of the most universally, and apparently best established properties of bodies can have no foundation in nature. These considerations are such that they are sufficient to throw a doubt upon the whole, and deter one from any further prosecution of a subject, where there is no little probability of success.

As it is not, however, because an opinion is received, but because it is well founded, that it is embraced by the philosopher, and as the properties which exist in nature will bear the test of experiments, however varied, it appeared proper to bestow a little more attention on the subject, and to try by some properly directed experiments, whether the weight is really increased, though the quantity of matter is diminished. The first consequence from this doctrine would be, that a metal in its perfect state, and in the form of a calx, should not fall through equal spaces in equal times. For the metal in its perfect state containing more matter, though it

weights less than the calx, is less acted upon by the power of gravity, and of consequence should descend with less velocity than the calx, the resistance of the air being taken away or made equal in both cases. The velocity with which a heavy body descends is most accurately determined by the motion of a pendulum. Sir Isaac Newton has demonstrated, in the 24th proposition of the 2d book of the Principia, that in pendulums, whose centres of oscillation are equally distant from the centres of suspension, the quantity of matter in each pendulum is in the compound ratio of their weight, and the duplicate ratio of the times of their vibrations in vacuo. And as a corollary from this, if the times of vibration are equal, the quantities of matter in the pendulum will be as their weight; and if the weights are equal the quantities of matter will be as the squares of the times of their vibrations or inversely as the lengths of the pendulums. By examining the times of vibration of two pendulums of equal lengths, the one made of a metal in its perfect state, the other of the same metal, in the form of a calx, we can determine whether gravity acts upon bodies in proportion to their quantity of matter.

Sir Isaac Newton, seems to have been the first that found out a method of accurately determining this point, and he tells us in the 6th proposition of the 3d book, that with experiments made with this view upon pendulums of gold, silver, lead, glass, common salt, wood, water, and wheat, in which

the length, weight, figure, and resistance of the air, were equal, that they all vibrated in equal times, and that by these experiments he could have determined an alteration in the quantity of matter within one thousandth part of the whole. He does not, however, mention the method he took to determine accurately the centre of oscillation of these different pendulums, which is by no means an easy matter. A very small inaccuracy, in this respect, would have produced very great differences in the result of the experiments. While in these bodies the difference between the quantity of matter and the proportional weight might be less perceptible than in some others. For these reasons, it seemed proper to repeat these experiments with all possible attention, and that the centre of oscillation might perplex as little as possible, a pendulum was constructed, that whatever its figure or the distance of the ball from the centre of suspension might be, its centre of oscillation still remained unaltered, and it still vibrated in the same time.

The principle upon which this pendulum was constructed was the same with that of the parallel rule. Two rods of straight grained wood were suspended at an equal height from the ground; the points of suspension were at the distance of eighteen inches. Each of these rods, at the lower end, had a piece of brass with a slit in it fitted to them, through which an iron bar passed and rested on them, by means of two wedges similar to those which sup-



port a common balance. The rods were parallel to each other, and of consequence, their distance from each other below was equal to that above, or eighteen inches, and this was the length of the iron bar betwixt the two wedges, on which it rested. To the middle of this bar was fixed a rod of iron, perpendicular to the bar, and parallel to the rods of the pendulum. Upon this a ball similar to that of a common pendulum was fixed: this was six inches in diameter and two inches thick in the middle, and could be moved upwards or downwards upon the iron rod. By experiment, this pendulum vibrated in the same times in whatever part of the perpendicular rod the ball was fixed, so that the centre of oscillation could not be altered by moving the ball of this pendulum, as it is in a common one. The ball of this pendulum was composed of two lenticular plates joined by their flat edge with screws. This ball contained the substance, of which the times of vibration were to be examined, that the resistance of the air might be in every case the same. The pendulum, thus constructed, was always of the same length, whether its ball were filled with the substance examined, or only half full, and the resistance from the air equal in all cases.

This pendulum I intended to apply to a clock, which was so made, that the power by which it maintained the vibration of the pendulum was not subject to alteration, from any difference in the maintaining power of the clock, or consistence of the

oil. And that no irregularities might happen, from the effect of heat on the rods of the pendulum, I proposed, by means of a stove, to keep the room where the clock stood as nearly of the same temperature as possible. The time kept by the clock was to be examined by the transit of a star, through the cross hairs of a telescope fixed firmly in one position. If this clock went faster, when its ball contained red lead, than when it contained lead, then gravity does not act upon bodies in proportion to their quantity of matter. And it is evident by this contrivance, much smaller differences in this respect might be observed, than by any experiment; however carefully made, with common pendulums. I expected before this time, I should have been able to relate the success of these experiments. But I was disappointed in the clock. The clockmaker, who got it to make some alterations to fit it for these experiments, did not return it in sufficient time. I made a few experiments with the pendulum alone, but have not determined any thing satisfactory. By a number of trials, when the ball contained four pounds of lead, it vibrated 172 times in three minutes measured by a stop watch, or  $57\frac{1}{3}$  times in a minute. When the ball contained four pounds of red lead, the difference of the number of vibrations in three minutes was not perceptible. But in a greater number of vibrations, perhaps, it might be more remarkable.

But supposing the experiments should succeed, it might still be urged against gravity acting upon bodies in proportion to their quantity of matter, that, though all bodies descend through equal spaces in equal times, where there is no resistance, this is no proof of their quantity of matter being in proportion to their weight; for the *vis inertiae* of a body may alter in the same manner as the gravity. For though the calx be heavier, and contains less matter than the metal, and falls through equal spaces in equal times with it, this may be owing to the *vis inertiae* being increased in the same proportion with the gravity. And the same principle which diminishes the weight, while it restores the calx to the metallic form, may diminish the *vis inertiae* exactly in the same proportion; so that the gravity will appear in both cases to be the measure of the quantity of matter, so far as this can be determined by the descent of heavy bodies, and the motion of pendulums. But if this be the case, inactivity is not an infallible characteristic of matter, since this can be supposed to be made greater or less at pleasure, and that of itself it has a greater disposition to motion at one time than it has at another. This is an opinion, which brings with it very serious consequences, which are left to those who know them, or can explain them better. One, at least, may be mentioned; it supposes matter capable of self-motion.

It may be attempted to reconcile these opinions by saying, that there is a fluid in nature which is subject to gravitation, and so subtile, that it pervades the pores of glass, or even of the densest bodies; that this fluid unites with metals during their calcination, and with inflammable substances during their burning, and is the cause of the increased weight observed in these experiments; and that the same fluid unites with the metal during its solution in acids. But whatever degree of probability there may be of the existence of such a fluid from the phenomena of light and electricity, yet the prodigious quantity of it that must necessarily unite with the metal, in order to produce this increase of weight, cannot easily be conceived without giving it some other properties, which would necessarily make it shew itself in a more remarkable way than it does. And as this fluid must exceed even the electrical fluid in subtilty, it should not, any more than that, increase the weight of those bodies on which it is accumulated. Now the electric fluid, by passing in considerable quantity through the calces of certain ~~metals~~ metals, restores them to their metallic form, and gives them the power of effervescing with acids, which they had not before the shock, and at the same time diminishes their weight, as I found by experiments made on this subject many years ago.

## NOTE BY THE EDITOR.

It can hardly be an object of attention at this period of chemical knowledge, to detail the cause of the phenomena considered in this essay, according to the explanation afforded by the antiphlogistic theory. I need only observe, that the discovery of oxygen gas immediately led the way to point out the source of the increased weight in metallic calces and burnt bodies. This essay, notwithstanding, in my opinion at least, shews the progress of men's minds towards that discovery, and affords a good view of the history of the transaction previous to the period of the proposal of the present theory. It is not necessary to make any remark on the reasonings on the quantity of matter in bodies which are equally just now as they ever were, but there are allusions to experiments performed by Dr. Irvine upon the increase of weight of metallic bodies by solution in acids, and precipitation by caustic alkalis, and also upon the increase of weight of the whole mass of acid and metal during solution, notwithstanding the emission of copious vapours. I shall now proceed to give a sketch of such of these as I am in possession of.

## EXPERIMENTS

*On the Alteration of Weight of the whole Mass of Metal and Acid, when metallic Substances dissolve in Acids.*

In these experiments, the metal, acid and vessels were first weighed with accuracy, the metal was then introduced into the acid, and after their mutual action was finished, the process of weighing was repeated, and the loss or gain estimated from the comparison of the sum of the weights of metal and acid before solution, and the sum of the weights after solution.

1. Two drachms of clean iron wire were dissolved in weak vitriolic acid; after solution and emission of vapours, two grains were found to be lost of the gross weight.

2. Two drachms of clean iron wire were dissolved in weak muriatic acid, and three grains and a half were missing.

In both these experiments, the heat and emission of vapours were considerable. They were both repeated with exactly the same success.

3. Two drachms of mercury were dissolved in diluted nitrous acid, and there appeared an increase of nearly a grain in the gross weights.

4. Two drachms of mercury put into three drachms of aquafortis, after their action had ceased, appeared to have gained in their joint weights a grain and nearly a half.

5. Two drachms of zinc were dissolved in two drachms of vitriolic acid, diluted with twice its weight of water, and the compound had gained nearly a grain, though the heat and emission of vapour were considerable.

6. Two drachms of zinc were dissolved in three drachms of aqua fortis, diluted with twice its weight of water, and a grain of the gross weights was lost.

7. Two drachms of bismuth were dissolved in half an ounce of aquafortis; there was neither gain nor loss, the balance remaining in the most exact equilibrium.

8. Two drachms of bismuth were dissolved in half an ounce of aquafortis, diluted with half an ounce of water, and two grains appeared to have been gained.

Experiments were likewise made on all the remaining common metallic substances, except gold, silver, and copper.

Several of these experiments were repeated by Dr. Black with the same success. He dissolved half an ounce of silver in an ounce of nitrous acid of the aquafortis strength, and he found it gained ten grains. It is not easy to dissolve tin and antimony in aqua regia, and iron in the nitrous acid so slowly and with the emission of so few fumes as these experiments require; lead does not dissolve readily in a weak nitrous acid without heat.

Though the experiments now stated do not by any means prove that the increase of weight in metallic oxydes arises from the removal of a principle of levity, yet they seem to be very



singular in their results, and to shew that the air must afford some part of its composition to the dissolving materials. This too, must be much more considerable than appears at first sight, and must amount not only to the weight of the few grains gained, but to the weight of all the fumes and vapours besides. This principle attracted from the air of the atmosphere, can hardly be any other than oxygen gas. In the cases of solution by nitrous acid, the azote cannot be supposed to become deficient, and there is no pretence for such a supposition, when other acids are employed: neither does there appear any reason to attribute the increase of weight to moisture absorbed, since these substances do not appear in other circumstances to be of a deliquescent nature, or to have an unusual attraction for water. The absorption of oxygen in these cases appears to resemble the instances of affinity which are distinguished by the appellation of predisposing affinity: for neither the acid nor metal separately exposed to the air is increased in weight. Now this may happen in two ways; either the acid unites with the metal in the state of what Dr. Thomson calls a protoxyde, and this compound greedily absorbs oxygen wherever it can meet it, or the nitrous gas in the case of nitrous acid, and the hydrogen, when other acids are employed, in their nascent state, are again attracted by the oxygen of the atmosphere, and nitrous acid or water formed again. Either of these processes taking place would account for an increase of the gross weights notwithstanding the emission of fumes. In the case of nitrous acid perhaps it might be determined to which of these the increase was owing. For if the access of oxygen were prevented it would be easy to observe whether the acid would dissolve the same quantity of metal as when exposed to the action of the air. If it did clearly the oxygen attracted by the nitrous gas could not have been the source of the increased weight. It is not so easy to give a rationale of the experiment mentioned in this essay, where a bottle was used provided with a valve which prevented the access of common air, and notwithstanding this the weight was very considerably augmented. If we could really suppose the valve to have acted its part with accuracy, and that

no oxygen found its way, the experiment would be one of the most surprising and unaccountable any where related. But one cannot proceed to form theories till such a thing be more fully established.

I am not exactly aware how far this subject has been investigated by actual experiment. But I believe the acetous acid is supposed to derive assistance from the atmospheric oxygen in the solution of copper and lead, as it appears clearly from no solution of these metals taking place when vinegar is boiled on them, the steam preventing the access of air. Such a solution, however, immediately commences when the materials are cold, and the air has free admission. I do not know whether it is exactly understood that the air influences the common solutions of metals in other acids as the experiments above related tend to shew. But it certainly is not known that the gross weights of the substances employed are augmented in the manner here stated. The muriatic acid also resembles the acetous, in refusing to dissolve some metals when the access of air is prevented.

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## ESSAY XIV.

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### ON WATER.

A. D. 1771.

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THE curious experiment of Van Helmont, since repeated with more accuracy by others, has been considered as a proof that water could not only be converted into earths, but into oils, resins, gums, and saline substances. This, however, must not be admitted without considerable limitations. That vegetable bodies draw scarce any thing from the soil in which they grow is confirmed by many experiments, and that soils differ principally from each other in the readiness with which they part with or retain water, and in the easiness or difficulty with which the roots of vegetables pervade them, or are supported by them. Van Helmont planted a willow, weighing fifty pounds, in a pot of earth, which he had previously dried and weighed, and covered with

a lead cover, so that nothing could be admitted to it but water, which he conveyed through a tube fitted on purpose in the lead cover, and which was at all times shut, except during the watering of the plant. At the end of five years he took off the cover, and separated the plant carefully from the earth. The pot, with the earth, after being well dried, had lost two ounces of its original weight. The plant weighed 169lbs. 3oz. or had gained 119lbs. 3oz. This increase of weight he supposes was owing to the water having been converted into all the parts of which his willow was composed.

Mr. Boyle repeated Van Helmont's experiment; he put the seed of a gourd, which does not weigh above a grain, into a pot of earth dried, and prepared as in the former experiment. A plant was produced that weighed 14lbs.; the earth had lost nothing. This experiment seems to prove, in the strongest manner, that none of the soil in which the vegetables grew had entered into its composition. Many experiments of the same kind made by M. Bonnet, of Geneva, shew, that vegetables will grow not only to a great size, but to the utmost perfection in the purest water. Anemones, tuberoses, ranunculuses, grew in distilled water to as great perfection, in the variety and beauty of their colours, in the elegance of their shape, and fineness of their flavour, as they did in the best gardens, or in the fields in which they grow naturally. Every body has seen the narcissus, the hyacinth, and other bul-

bous rooted plants growing in water. He even made vines vegetate in water, and found the plant not only grow with such vigour, as to send forth shoots of ten or fifteen feet long in a year, but it produced most excellent grapes. The lemon and the orange tree produced the finest lemons and oranges : the liquorice and the gentian, placed in the purest water, were still found to grow with vigour, and to have the sweet or the bitter juices peculiar to them. From these experiments it appears, that water is capable of assuming a variety of forms. The same pure water, merely by passing through the vessels of different vegetables, has been converted into the sour juice of the lemons, the bitter of the gentian, the sweet of the liquorice, the fine agreeable flavour of the tuberose, and the disagreeable one of the rue. Besides being converted into the gummy, the resinous, the saline, or oily parts contained in vegetables, it has been converted into wood, into bark, into leaves, and into flowers. No substance that we know of is capable of assuming such various forms and qualities as the water seems to have done in these experiments.

Though pure water thus apparently assumes the forms of gummy, resinous, or of oily substances, we must not, with Van Helmont, allow that they are entirely composed of it. Experiments indeed shew that they contain a great proportion of water ; but experiments shew likewise that they contain something else. And though vegetables seem, from the experiments that have just been mentioned, to



## ON WATER.

pure water alone, something else is necessary  
1       vegetation, without which, they either will  
      not at all, or if they do, they no longer have  
      difference in their taste and smell which they nat-  
2       possess; but all of them contain the same  
      watery juice very little different in all vegetables.—  
Every body knows that the great promoters of vege-  
tation are a proper degree of heat and moisture, and  
a constant admission of fresh air. The seed of a  
plant in the best earth, and with plenty of water,  
will not grow in an exhausted receiver, whatever de-  
gree of heat it may be exposed to. And Dr. Hales  
has shewn, that air is contained in all vegetable pro-  
ductions, and is constantly absorbed by vessels destin-  
ed for that purpose, and is as necessary for their ex-  
istence as air is to man.

But we find that vegetables, though furnished with  
air, water, and heat, may be placed in situations  
where they either scarce grow at all, or grow very  
weakly, and never arrive at any perfection. A ve-  
getable, in a dark box, constructed so that the air  
may easily be admitted to it, grows slowly, but it  
never contains any thing but a watery juice. Hence,  
the rays of light are somehow necessary, for the per-  
fect growth of vegetables, since, when deprived of  
this, they all agree together in the nature and quali-  
ties of the juice which they contain. We no longer  
find them capable of producing juices of such diffe-  
rent qualities: they no longer have that variety in  
their colour and flavour that they had before. The

most pungent vegetables grow insipid ; the highest flavoured inodorous, and the most variegated in their colours are of an uniform white when excluded from the light. Vegetables which grow in a natural situation readily burn when dry. A vegetable bred in a dark box contains nothing inflammable: that something is attracted from the light seems therefore probable.

This is not peculiar to them: some other substances, particularly solutions of metallic bodies in acids, upon being exposed to light, attract something from it. I find a quantity of copper, put into the muriatic acid in a dark place, remains perfectly colourless, though kept for a year in that situation: upon being exposed to light in a glass bubble, hermetically sealed, it first became yellowish, then of a dark brown ; in a day or two of a beautiful green ; while the rest of the solution, kept in the dark, is still as colourless as water. This change cannot be accounted for from the air, for the bubble was exhausted of air: it must have proceeded from the absorption of some very subtile matter which easily pervades the glass. This principle, which vegetables attract from the light, mixed with the air they absorb, and united with the water which they draw from the soil, is probably the cause of the difference betwixt the juices of vegetables and pure water ; and the differences between the juices of vegetables may arise from the mixture of these substances with each other in different proportions.

Whether the air which vegetables absorb is the common air which we breathe, or air which has undergone some change by contributing to the inflammation of combustible bodies, and the respiration of animals, has not been determined by experiment.—An immense quantity of the common air is every day altering its qualities, and, by contributing to inflammation and respiration, becomes at last unfit for these, and extinguishes flame and suffocates animals. In a little time the whole air would be thus changed, were it not restored to its original state by some constant natural process. I imagine the vegetable kingdom performs this operation, though I know no experiment to prove it, nor is it easy to contrive one that would be conclusive. We have thus, by examining in a slight manner the process of vegetation, found that the opinion, of all the juices in vegetables being pure water, is without sufficient foundation, though water is the principal ingredient.

The waters of some springs are remarkable for depositing great quantities of earth on the bodies which they touch, and by which they are, in time, converted into stone. These petrifying waters have always been regarded as curiosities: but the earth which they deposit is not owing to the water being converted into earth, but to a quantity of earth which they contain in a dissolved state. These petrifying waters are not pure, but leave the earth which they contain behind them upon being distilled. The water that has the property of converting the sub-

stances on which it falls into stone, or which by constantly dropping from the tops of caverns, forms a number of pillars or stalactites, like icicles from a house in frosty weather, differ from one another in the quantity of earth which they contain. The spring in the wood at Hamilton, which petrifies the moss over which it runs, and one at Woodside, near Glasgow, contain but little earth. The one at Knaresborough, in Yorkshire, contains a great deal, so that it will harden a vegetable as if it were frozen in 24 hours, and will completely petrify it in two months. This is the most remarkable water for this property that is known: it runs over a rock about eighteen feet high, in a great number of small streams, which, falling on moss at the foot of it, have converted it all into stone. It has afforded matter of speculation to the chemists, to find out by what means the water has dissolved this earth. The petrifications of it, it is well known, all burn into lime. But the earth, in the Knaresborough water, cannot be in the form of lime, otherwise it should gather crusts in the air like lime-water, which I found by experiment it did not. We can scarce imagine that it is dissolved by a saline substance, because nothing of this kind is to be found in the petrification, which should, if this were the case, be the calcareous earth dissolved by some salt, and not a pure earth.

The reason why it has been commonly supposed that it was owing to a salt, is from the taste of the water, which is manifestly sour, with something of

an astringent taste, not unlike a weak solution of alum in water. Upon exposing a little of the water in a saucer, no scum or crust gathered upon it; but as the water slowly evaporated, a whitish powder fell to the bottom, which would not again dissolve in the water. A perfectly caustic alkali threw down a white powder, which neither would dissolve again; another proof, that the calcareous earth was not in this water in the form of lime. This experiment seemed to prove that it was a salt that kept the earth dissolved; and yet no salt being found in the petrification, seemed to contradict this.

Air that has passed through the fire, or been breathed by animals, or which is separated from calcareous earth or alkalis, during their solution in acids, readily is absorbed by water. The water thus saturated with air has the exact taste of Knaresborough water, only a little stronger. It is very sour, changes the blue and purple vegetable infusions to red, like the acids, and exactly resembles the water of Pyrmont, which seems to contain this air only, or this and a little iron. An experiment made with this water, saturated with air, by Mr. Cavendish, mentioned in the Philosophical Transactions, suggested an explanation of the Knaresborough water. This water, mixed with lime water, throws down the lime by furnishing air to it, and reducing it to its natural insoluble state: but two parts of water, and one of lime water, upon being mixed at first are milky, but the transparency is recovered by shaking. This experiment

is not easily explained, so as to agree with the opinion of the calcareous earth being soluble in water, in consequence of its being deprived of air ; for the lime had attracted the air at first from the water, as is evident by its becoming milky ; yet it was even in this state instantly redissolved.

I made some experiments upon this mixture, and the only probable account I could fall upon was, by supposing that the calcareous earth was not only soluble in water, when deprived of air, but also when it contained too much of it : and that it was least soluble in an intermediate state. This mixture of lime-water with water saturated with air, agrees almost exactly in all its properties with the Knaresborough water. That the calcareous earth is dissolved in the mixture of lime-water and water saturated with air seems to be proved by the following experiment : a caustic alkali, which did not at all affect lime-water, turned this muddy, and the calcareous earth soon fell to the bottom. Upon examining it, I found that it agreed in every thing with chalk, and like it effervesced with acids. The earth was, in this case, precipitated by the caustic alkali attracting the redundant air, and reducing it to the middle state in which it is least soluble. For this the alkali has a stronger attraction than the calcareous earth ; but the calcareous earth has a stronger attraction for the remaining air, as is evident from Dr. Black's experiments. A caustic volatile alkali, which has still less attrac-



tion for fixed air than the fixed alkalis, has the same effect, and for the same reason.

If this explanation were just, two consequences necessarily flowed from it: 1, That an alkali, already saturated with air, should have no effect upon the mixture of lime-water and water saturated with air, because it could not attract this redundant air from the calcareous earth, so as to reduce it into its middle insoluble state: 2, That chalk should dissolve in water saturated with air. These were found to be true upon trying the experiments. Common alkalis will not answer, as they are far from saturation: they must be quite saturated, and when in this state, the transparency of the liquor is not in the least affected by them. This may, therefore, be used as a test for a mild alkali, as lime-water is used as a test for a caustic alkali. The mixture of chalk and water saturated with fixed air is exactly like the Knaretsborough water.

The water of the Knaretsborough spring seems to be produced thus. A quantity of air of this kind is found in caverns underground as in coal-pits, which cannot be wrought, on account of its extinguishing the candles of the workmen, and being totally unfit for their respiration. If this air is in contact with the water of any spring, a part of it will immediately be absorbed; the water will acquire a sour taste, and will sparkle in the glass like the Pyrmont water. If this water, in its course, meets with a quantity of

lime-stone or chalk, it will dissolve a small quantity of them, and form a petrifying water, in every respect like the Knarsborough water.

The water of all springs is impure, compared with rain or snow-water; they all contain something of a saline ingredient. As rivers arise from springs, and flow over substances soluble in water, they likewise contain saline matter, besides a quantity of putrifying, animal, and vegetable bodies, which they meet in their course, and a part of which they dissolve.

The saline substances which is found in the earth, in the greatest quantity, is common salt. Immense beds of it are found in different parts of the world; chains of mountains, even some islands, are almost entirely composed of it. No substance is on this account so common in water as sea-salt; no water is entirely free from it. Water is a very volatile body; a very moderate degree of heat is sufficient for converting it into vapour; even in the solid form of ice it evaporates, though more slowly than in a fluid form. The saline substances cannot be converted into vapour by any degree of heat which naturally happens in any of the climates of this globe. The water in rivers is therefore constantly evaporating during their course, while the saline parts remain. The proportion of the saline parts to the water will therefore be constantly increasing, and will be greatest in the lowest part of the globe. Hence, the water of the ocean contains more salt than any other water; and those parts of the ocean that are exposed

to the greatest heat will be saltier than the rest. The sea water, under the equator, contains more salt than in higher latitudes.

The water which is converted into vapour is free of every thing saline ; and by being driven about by the wind, and attracted by the high mountains, falls down in the form of rain, dew, hail, and snow, which supply the fountains, and these the rivers, which again fall into the sea ; so that the water is subject to a perpetual circulation, by which it affords new support to the vegetable kingdom, exhausted by the evaporation of its watery parts. In consequence of this constant evaporation, the fresher water towards the poles must tend in a constant current towards the equator, otherwise the water at the equator would, by this time, have been entirely evaporated, and the whole converted into solid salt. That there is a current of this kind, is confirmed by the accounts of navigators. As the evaporation goes on principally at the surface, the water at the surface will soon become saltier than the rest ; but at the same time it becomes heavier, so that it falls in a constant stream from the top towards the bottom. Experiment confirms this, for the sea is saltier at the bottom than at the surface.

If a large bay is formed by the sea, which communicates with the rest of the ocean by a narrow neck, the current produced by the evaporation will be easily perceived. The Mediterranean is of this kind, which extending to a surface of 300,000 square miles, comes

communicates with the Atlantic at the Straights, by a neck about five English miles over. Through this there is a constant current, it is affirmed, of two miles in the hour. This Dr. Halley long ago accounted for from the evaporation from the surface of the Mediterranean being greater than what it receives from all the rivers which fall into it, many of which are very large : but if this sea had lost annually, since its existence, the quantity of water which Dr. Halley supposes, it would long, ere now, have been converted into a vast mass of indurated salt. By calculations from the quantity of salt contained in the water of the Mediterranean, it appears, that in 500 years, the salt separated would have been 250 feet high. Now the Mediterranean is, in many places, not of this depth. So that this sea would have been wholly changed into salt, if the salt-water continually flowing into it had no issue : but in all the time it has been known it has not become saltier.

The explanation seems to depend on the water in the Mediterranean being saltier than that in the Atlantic. By experiment, it is said the former contains near  $\frac{1}{7}$ , the latter contains  $\frac{1}{8}$  part of salt. The surface of the two seas, therefore, being of equal height, their gravity would not be equal ; so that the two seas would run together, the lighter spreading on the surface, the heavier spreading on the bottom. Hence there would be two currents, one from the Atlantic to the Mediterranean on the surface ; another from the Mediterranean to the Atlantic, on the bot-

room, in the same manner as in a warm room while the cold air rushes in at the bottom, the warm air flows out at the top of the door.

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### NOTE BY THE EDITOR.

THIS essay was publicly read in the year 1771 by its author, and ever afterwards formed a part of his course of chemical lectures. The records of the society to which it was communicated, and the recollection of many of its members of unblemished integrity and enlightened minds would readily, if necessary, give the most precise evidence of the correctness of the date, which is moreover affixed to the manuscript by the author's hand. This precision is requisite in this instance to shew that the observations contained in this essay on the solubility of chalk in water, impregnated with carbonic acid, were made without any knowledge of Bergman's experiments on the same subject, which did not take place, or at least were not published, till the year 1774. It is certainly very much to be regretted that my father was so extremely averse to commit his observations to the press, and it has been peculiarly prejudicial to his reputation as a chemist. It arose, however, I believe in a great measure from a nicety, perhaps a fastidiousness of taste, which rendered him always dissatisfied with the degree of perfection to which he had brought his speculations, and partly also from an opinion not very well founded that teaching his theories and experiments to his pupils was equivalent to a more formal publication of them.

It will not be here necessary to revert to the consideration of Van Helmont's and Mr. Boyle's experiments on the growth of plants in water, which have been noticed in a former note, nor to the purification of vitiated air by vegetables, and the decom-

position of carbonic acid gas, which I have already stated, Dr. Irvine thought himself to have first suggested.

There is an experiment mentioned in this essay, however, which deserves more attention. I allude to the solution of copper, made in the muriatic acid which was colourless as long as kept in the dark, but by exposure to light became first yellow, then brown, and finally green. It is a curious inquiry here, how did the influence of light produce this change of colour? There are two muriates of copper, the green muriate which contains a large quantity of oxygen and little copper, and the colourless muriates which contain less oxygen and more copper. This latter may, according to Proust, be obtained by plunging a plate of copper in muriatic acid, and keeping it for some time in that situation. It appears then that Dr. Irvine formed this salt, which when exposed to the air speedily attracts oxygen, and is converted into the green muriate. Pains, however, seem to have been taken to guard the solution from the contact of air. If the effect thus be produced by oxygen, and the oxygen was not derived from the atmosphere, it must have come from the water, and that by the influence of light in some manner. The muriate of copper may not attract the oxygen of the water so strongly as the hydrogen does; but the light having access, ~~may attract the hydrogen~~ with a certain force, which may so weaken its affinity for the oxygen that the water may be decomposed, and the muriate become green. But if this be true, what becomes of the hydrogen? It may be answered, that it may assume the gaseous form; or if the recent discovery of the composition of muriatic acid should be confirmed, the hydrogen may be supposed to be altered in another way, and combine with the solution in the form of muriatic acid. It seems not improbable, from the various changes of colour of the solution, that copper is capable of combining with oxygen in more than two or three proportions.

I find many records of experiments upon the water of different springs and wells by Dr. Irvine; in particular the water of Glasgow, where he resided, attracted his attention, and I subjoin a



### ON WATER

It is contained the results of many trials made to  
the comparative purity of water from various wells,  
springs, there and in other places. A bubble was  
filled with water at 56°, and the temperature was noted  
at which the same bubble sunk in various kinds of less pure  
water.

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		Degrees	
1	Distilled water	56	
2	Clyde - -	58	
*3	- - -	62 $\frac{1}{2}$	
4	- - -	74	
5	- - -	73 $\frac{1}{2}$	Temperature of the well, July 29, 1776 = 52°
6	- - -	83	
7	- - -	64	
8	- - -	66	
9	- - -	67	
10	- - -	62 $\frac{1}{2}$	
11	- - -	64 $\frac{1}{2}$	Temp. of the water in the well = 52°, 1776.
12	- - -	58	Temp. ditto = 52°, 1776 = a gallon of Arn's well water, left a residuum of only two grains upon distillation.
13	- - -	68	Ditto ditto = 52.
14	- - -	68	
15	- - -	62	Temp. July = 50.
16	- - -	70	
17	- - -	65	
18	- - -	59	
19	- - -	59	Temp. in well = 46°, Feb. 1780.
20	- - -	60 $\frac{1}{2}$	Ditto = 44°.
21	- - -	62 $\frac{1}{2}$	Temp. 50°, March 1780.
22	- - -	64	Temp. 50°. This water contains iron, fixed air, and a little muriatic acid.
23	- - -	72 $\frac{1}{2}$	Contains iron.
24	- - -	69 $\frac{1}{2}$	
25	- - -	73	
26	- - -	64 $\frac{1}{2}$	
27	- - -	72 $\frac{1}{2}$	
28	- - -	73	
29	- - -	74	
30	- - -	70	
31	- - -	64 $\frac{1}{2}$	
32	- - -	64 $\frac{1}{2}$	
33	- - -	70	
34	- - -	63 $\frac{1}{2}$	
35	- - -	62	
36	- - -	68 $\frac{1}{2}$	
37	- - -	61 $\frac{1}{2}$	Contains iron.
38	- - -	59 $\frac{1}{2}$	
39	- - -	63	
40	- - -	62 $\frac{1}{2}$	
41	- - -	70	
42	- - -	78	
43	- - -	60 $\frac{1}{2}$	
44	- - -	59 $\frac{1}{2}$	
45	- - -	59 $\frac{1}{2}$	
46	- - -	94	
47	- - -	169	

\* 3. Dean-side well water. 4. Well in High-street, at the old venal. 5. Ditto in Mr. Thompson's close. 6. Old well in Parlanes close, new opened and ordered to be shut. 7. Bell's wynd

This table affords pretty fair grounds to determine the degree of impurity contained in any of the waters examined, but not of the kind, unless where particularly specified. The best water is the lightest, and consequently the bubble that rises in the purest water will form in more contaminated sorts. Therefore, the lower the temperature at which the bubble sunk in any of the experiments above stated, the purer is the water. The use of applying heat was to render the water, bulk for bulk, lighter than it otherwise was, so that it might sit the bubble. The number of degrees in the above table then may be considered as indicating the purity or impurity of the different waters, the smallest number making the least degree of contamination.

This essay is not complete. I have abridged its length considerably, by omitting a pretty long discussion on the appearance of earth in frequently distilled water, the source of which is now better understood.

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ditto. 8. Westport well. 9. Western sugar-house close ditto. 10. Jock factory well. 11. Ram's horn well. 12. Arn's well. 13. Salt market well. 14. Well in Down's close, Gallougate. 15. Well of G. Crawford, Esq. 16. Well in Salt market below Bridgegate. 17. Mr. W. Cunningham's well, Queen-street. 18. Ditto from Lenshaw. 19. St. Thomas' well, Poisl. 20. St. John's ditto. 21. Lady well. 22. Phytic well. 23. Spoutmouth well. 24. Well opposite the guard. 25. Tron gate well, opposite the Exchange. 26. Mr. Shortridge's well, Dunlop-street. 27. Blue pump, Argyle-street. 28. Red pump, near the head of St. Enoch's Wynd. 29. Hornscourt. 30. Well in Mr P. Colquhoun's close. 31. Well fronting Buchanan-street. 32. Well in Mr. Al. Gordon's close. 33. Adam's-court. 34. Well opposite Jamaica-street. 35. Mr. Young's, the bleacher, in Buchanan's crooked wynd. 36. Mr. A. Buchanan's, Jamaica-street. 37. Enoch-bank. 38. Spring bank. 39. Ditto well. 40. Two wells at Smithfield. 41. Water from the town hospital well. 42. Stock well, Oswalds close. 43. Gourrock Bore well. 44. Greenock cistern. 45. Port Glasgow ditto. 46. Iron spring from Sanghuar. 47. Sea water at Fairlie, in the Frith of Clyde, in dry weather.

**PART THE THIRD;**

**CONTAINING**

**EXPERIMENTAL ESSAYS.**

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**BY**

***WILLIAM IRVINE, M. D.***

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***“ Et quorum pars magna fui.”***



## ESSAY I.

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### *ON LATENT HEAT.*

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IT has been mentioned in the first part of these essays that the science of chemistry owes to Dr. Black the discovery of latent heat. That philosopher first pointed out the entrance of a large quantity of caloric into all bodies during their conversion from the solid to the fluid form, as well as during vaporisation, or the change effected by means of heat, from fluidity to the aëriform state. All men are now agreed in attributing a due and ample share of merit and importance to this most ingenious and admirable theory, which has been the ground-work of almost all the recent advances of chemical knowledge.

Dr. Black having conceived the probability of the existence of latent heat resolved to confirm his opinion by a reference to actual experiment. He



preferred for this purpose the case of ice and water, the most familiar in these latitudes to the observation of men, and infinitely the most important of all, from the great scale upon which it is effected by nature. It is far from my intention to detail here all the experiments which Dr. Black made to determine this point. One instance will be sufficient. If there be mixed in a proper vessel a pound of ice at  $32^{\circ}$  with a pound of water at  $172^{\circ}$ , the whole becomes instantly fluid, and the temperature is  $32^{\circ}$ . Hence the water was cooled  $140^{\circ}$ , and the ice not heated at all, but only melted. One hundred and forty degrees of heat, therefore, have entered into the ice during its fusion, and are necessary for producing that effect. That this is really the case appears plainly from repeating this experiment with a little alteration. If, instead of the pound of ice at  $32^{\circ}$ , we substitute a pound of water at  $32^{\circ}$  the temperature of the mixture is not  $32^{\circ}$  but  $102^{\circ}$ , the surplus of heat above  $32^{\circ}$  being shared equally by the two portions of water.

Having proved in this manner that ice does not refuse to admit heat, while fusing, but that  $140^{\circ}$  are required to convert it into water, Dr. Black proceeded no further by experiment. He extended his theory, however, to all cases of fusion whatever. The  $140^{\circ}$  which are thus demonstrated to exist latent in water, are measured by the capacity or specific heat of water, as is evident from the consideration of Dr. Black's experiments. For, in order to fuse

the ice, an equal weight of water is deprived of  $140^{\circ}$  of heat, which are only sufficient to satisfy the demands of the new capacity. If, therefore, we suppose the capacity of ice for heat to be as nine, that of water being ten, and nobody has made the difference less, the latent heat of water, expressed as it is usual and convenient to do in all other cases, where it is extremely difficult, if not altogether impossible to ascertain the specific heat of the fluid in question, would be  $155^{\circ}.555$ , and this mode of expression would undoubtedly be more convenient than the present one, where the comparison of the latent heat of various bodies is to be made, if the capacity of ice were accurately known. Even with our present imperfect knowledge of that circumstance, I shall, in this essay, state the latent heat of water at  $155.555$  degrees measured by the capacity of ice.

It cannot afford just ground for wonder that there should still remain some doubt of the accurate specific caloric of ice, considering the extreme nicety of all experiments upon heat, not one of which, I may boldly affirm, can assert any claim to perfect precision. Many other philosophers have also repeated and modified the experiments of Dr. Black, to determine the latent heat of water. And though the case is as simple and favourable as any case can be, the results have by no means exactly coincided with each other. To illustrate the degree of uncertainty of such determinations, it may be worth while

to compare the numbers found by Black, Wilcke, and Laplace, three of the most distinguished philosophers who have turned their attention to this subject. It is stated in the *Annales de Chimie*, vol. 3, page 171, that the latent heat of water, as determined by Dr. Black, is, in degrees of Reaumur,  $62^{\circ}.22$ , as found by Wilcke  $57^{\circ}.73$ , and by Laplace  $60^{\circ}$ , which numbers may be roundly given, according to Fahrenheit's scale, as 140, 130, and 135. Mr. Nicholson, in his *Treatise on Chemistry*, states the latent heat of water at a number considerably higher than any of these,  $146^{\circ}$ ; but I have not been able to find upon what authority, though I have no doubt of that gentleman's accuracy. Upon the whole there seems fully as much reason to trust to Dr. Black's determination of this point, as to those of the other philosophers; and the numbers, in general, do not differ more from each other than it is reasonable to expect they should, in all cases of so much delicacy. For it never ceases to be necessary to recall to the recollection of the reasoner upon the theories of caloric, that all the numbers expressive of the capacities and latent heats of bodies are only approximations to truth, from which they even sometimes widely diverge.

The only other philosopher, as far as I know, who has attended to the precise quantity of the latent heats of different substances, is Dr. Irvine. Landriani, it is true, made some experiments to prove that the fluidity of alum, sulphur, and some metals was

accompanied with an absorption of latent heat ; but I believe he made no attempts to ascertain the precise quantity. Dr. Irvine investigated this subject soon after Dr. Black, and the substances submitted by him to examination were spermaceti, bees' wax, and tin. He found the latent heats of these bodies to be as follows :

Spermaceti.....	145°
Bees' wax.....	175°
Tin.....	500°

From the imperfect notes which I possess, of the methods used to determine the two first of these, the 145° are measured by the capacity of melted spermaceti, and the 175° by the capacity of melted bees' wax. I am perfectly ignorant of every circumstance regarding the latent heat of tin, as determined by my father, excepting the number itself, as I have been unable to find any notes of his experiments for ascertaining this point ; but it is reasonable to conclude that these 500° are measured by the capacity of solid tin, because it seems an attempt of great difficulty, if not altogether impossible, to find the capacity of that body in its fluid form. The probability of this is considerably augmented by the result of some experiments which I lately undertook to determine again the latent heat of tin, and in which the numbers agreed very nearly with that here stated, on the supposition that the degrees were intended to be applied to tin in its solid state.

It occurred to me, some time ago, that it might be possible to add a little to the information at present possessed concerning the quantity of latent heat necessary for the fusion of various bodies. Whatever opinion may be entertained of the nature of latent heat, or of the manner in which it enters into bodies, there can be no doubt of its existence in many cases. It appears therefore, an object of some curiosity to compare its quantity in various instances, and to trace, if possible, any principle by which its entrance into fusing substances may seem to be governed. But with our present scanty knowledge of this subject, such an attempt is not likely to prove successful. There are only four instances in which the latent heat has been attempted to be ascertained, and though the subject is surrounded by considerable obstacles, yet these do not always appear to be of an insurmountable nature. Influenced by these considerations, I resolved to endeavour to extend our knowledge a little upon this subject, and, in consequence, made a great number of experiments, of which I now proceed to give some account,

The vessels which I employed were extremely simple, and consisted of nothing more than common Florence flasks of which the necks were cut off at a convenient distance. In these the water made use of was contained, and the vessel was supported on a slight wooden stand, which presented a very small surface to abstract heat from the materials. The orifice at the superior part of the vessel, was, in general, not

more than sufficient to permit the ready introduction of the fluid examined; probably from an inch and a quarter in diameter to a little more, so that a very small surface of the water was exposed to the air. The weight of the glass was, in all cases, accurately ascertained. These circumstances being premised, I proceeded in the following manner.

The first substance which I submitted to examination was bismuth. The melting point of this metal was, by the thermometer which I used,  $480^{\circ}$ . This is so very near the point found by my father, to wit, 476, that I shall consider his determination as the true one, as he combined and compared the different ways of computation and observation, and I know the thermometer which he employed to have been made with particular care. Into a glass vessel which weighed 411 grains, and which was constructed and situated as above mentioned, I put 2236 grains of water, of the temperature of  $62^{\circ}$ , which was that also of the apartment in which the experiment was conducted. I then removed from the fire a quantity of fluid bismuth in a crucible; I waited till it was partially solid, and the solid part projecting into the fluid; at that moment I inferred that both the solid and fluid metal were of the common temperature of  $476^{\circ}$ . This, I believe, is a fair induction. It is impossible to spare time during the experiment, to ascertain the temperature of the femiliquid. But the melting point of any substance can be ascertained in no other way than on



the supposition that a solid immersed in its relative fluid is either melted by it, or if it remains in the fluid unmelted, and the fluid is not frozen, that both are at one temperature, and that temperature is the melting point. A thermometer introduced into a crucible filled with a mingled mass of solid and fluid bismuth always points to  $476^{\circ}$  or very near it. I proceeded to pour a portion of the fluid bismuth into the vessel of water. The mixture was well stirred, and in one minute the thermometer stood in it at  $86^{\circ}$ ; in two minutes at  $85^{\circ}.75$ . The true temperature at the moment of mixture was, therefore,  $86^{\circ}.25$ , supposing equal temperatures to be lost in equal intervals. There was also a small quantity of steam formed. I now weighed the vessel with its contents of water and bismuth, and found that it had gained 1555 grains. This, therefore, ought to have been the quantity of bismuth poured in, if there had been no loss. But upon drying and weighing the metal, it appeared to amount to 1589 grains, and 34 grains of water or metal have consequently disappeared. The bismuth was cooled  $389^{\circ}.75$ ; the 2236 grains of water were heated  $24^{\circ}.25$ . Then 1559 grains of water, or a quantity equal to that of the bismuth, would have been heated more in the proportion of 1559 to 2236, or  $34^{\circ}.77$ . These  $34^{\circ}.77$ , in order to be compared with the degrees which the bismuth has lost must be reduced to the same value, or that quantity of heat which they contain must be expressed by degrees measured

by the capacity of bismuth. This will not alter the quantity of heat, but only give us a different expression of it. The specific heat of bismuth is stated in Thomson's System of Chemistry at .043: the quantity of caloric which raises the temperature of water  $34^{\circ}.77$ , would therefore raise that of an equal quantity of bismuth  $850^{\circ}.46$ . But the bismuth after becoming solid, could only have lost  $389.75$ , which being subtracted from  $850^{\circ}.46$  there remains  $460^{\circ}.71$ , which cannot be accounted for by the cooling of the solid bismuth, and which must consequently be the whole or a part of the latent heat of the fused metal.\*

But the latent heat must be greater than this; for 411 grains of glass were also heated  $24^{\circ}.25$ . If the capacity of this be taken at .174, as Mr. Kirwan found flint glass to have, and I have found green bottle glass to have a capacity .173 by several experiments, whence it is probable that the glass of Florence flasks has its capacity not very wide of these numbers; if then .174 be taken as the capacity of this glass, these  $24^{\circ}.25$  which the bismuth communicated to the 411 grains of glass in the vessel, when measured by the capacity of the bismuth, are equal to  $98^{\circ}.1$ : and 1559 grains of glass would have gained  $25^{\circ}.8$ , which must be added to  $460^{\circ}.71$

\* Some errors, which however were merely of an arithmetical kind, will be observed to have escaped me in the hurry of writing this train of reasoning when it appeared formerly. These are now corrected.

already found, and make  $486^{\circ}.51$  for the latent heat of bismuth.

But this is obviously still too little ; for, as has been already mentioned, there was a good deal of steam formed. The amount of the heat thus lost, it is extremely difficult to assign. I shall, however, make an attempt to guess at, rather than determine it. All the 34 grains must not be reckoned to have been lost in evaporation. In spite of all my efforts I could perceive that some, though certainly a small quantity of bismuth, went off with the water, in the form of a number of very minute particles floating in the liquid : and, perhaps also, a small portion of water might be wasted during the process. If we allow 18 grains of the loss to be accounted for in these ways, there would remain 16 grains of water which have been converted into steam. The latent heat of steam was computed, by Mr. Watt, to be equal to  $940^{\circ}$ . But this steam cannot be allowed so much latent heat as this ; and though it may be difficult to point out the quantity to be fairly granted, yet I shall expect to be within bounds, when I estimate it at one-half of  $940^{\circ}$ , or  $470^{\circ}$ . In that case we should say, sixteen grains of water have been heated  $470^{\circ}$  ; sixteen grains of bismuth would be heated by the same quantity of caloric,  $10930^{\circ}.23$ , and 1559 grains + 16 grains, supposed to have been poured off with the water, = 1575 grains of bismuth would be heated  $112^{\circ}.43$ . This quantity of heat, therefore, ought to be added to the

486°.51, already found, and would amount in all to 598°.94 which should be the whole latent heat of bismuth.

I am perfectly sensible that there are several gratuitous suppositions in this last part of the reasoning, where it is attempted to estimate the amount of the steam produced, and of its latent heat ; I do not, therefore, lay much stress upon it. The number 486°.51 is not at all affected by the truth or falsehood of the subsequent inductions, and, as far as I see, stands upon firm grounds. I endeavoured, however, to confirm or refute the truth of the last part of my argument by making an experiment exactly on the same principles, but where by dexterity, I might prevent the formation of steam wholly or in great part. In one instance I succeeded tolerably well, and then the latent heat, with every correction, amounted to nearly 600°, approaching thus extremely closely to the determination by computation.

Having mentioned the word dexterity, it is proper that I should give some explanation of what is meant by it here. Though it may seem, at first sight an excessively easy thing to pour fluid bismuth into water, yet it will be found, upon trial, to be a matter of considerable difficulty to do it in such a manner as to make the calculations agree in their results. This difficulty arises from the unequal quantity of steam formed by different management, and also from the water sometimes violently thrown

about by the metal, at the moment of its first contact with it. Sometimes a violent explosion takes place, occasioned, as I suppose, by a hollow sphere of bismuth being rapidly formed, inclosing a portion of water, which it suddenly rarefies, and expands with great force. I cannot, in all cases give directions how to avoid these accidents, but I know very well that they rarely or never happened during the latter part of my experiments. Some observations, however, suggested themselves to me. Care must be taken that the metal is not too near complete fixation, and that no portion of the solid metal be poured along with the fluid. Similar cautions must be observed with respect to the oxyde which is always found floating on the surface. The method of pouring out the metal also requires attention: if it be poured suddenly, or with a jerk, a large jet of steam is immediately thrown out as the metal touches the water: if even a small stream be poured in at one point only, the solid metal forms a column in the water which quickly reaches the surface, and at that moment an immense quantity of steam is formed. On the other hand, if the fluid metal be poured from too great a height, it may become solid before reaching the water, and thus defeat the intention of the experiment. A little practice, however, is sufficient to enable any one to avoid or lessen these sources of inaccuracy.

I repeated these experiments for determining the

latent heat of bismuth many times, and the result is expressed in the following

TABLE.

No. of Experiments.	Latent heat by the first computation.	Correction for the heat received by the glass	Whole latent heat.
1	457	23	480
2	411	29	440
3	412	28	440
4	465	33	498
5	480	29	509
6	438	27	465
7	465	30	495
8	460	25	485
Mean	448		476

On the whole, therefore, it appears that we shall not exceed the truth when we estimate the latent heat of bismuth at  $550^{\circ}$ .

I made, precisely in the same manner, two experiments to ascertain the latent heat of tin, and of these the results were—

Experiment.	Latent Heat.
1	520
2	495
Mean	507

This agrees remarkably well with the determination of the same point made by Dr. Irvine.



Zinc requires for its fusion a higher temperature than that at which mercury boils. The mercurial thermometer, therefore, cannot be used to determine the melting point of that metal. But it was computed by Bergman to melt at 700° of Fahrenheit's scale. Taking that for granted, I made three experiments on the latent heat of zinc, in a similar way with that already related with bismuth. These experiments might be easily detailed at full length, but it is to be feared that so tedious and minute an account might have a greater tendency to exhaust the patience, than to add to the information, of the reader. I shall therefore only give the results of the calculations instituted, which I have in this, and every other instance, taken pains to render accurate by repeated trials.

## EXPERIMENTS

*To determine the latent Heat of Zinc.*

Experiments.	Latent Heat by first computation.	Correction for Heat received by the glass.	Whole latent Heat.
1	490	28	518
2	476	22	498
3	443	32	475
Mean	469		497

In these experiments no allowance is made for the heat lost by steam, which, however, by the dexterity acquired by practice, I was enabled to render very small.

Lead I found to melt at a point above  $584^{\circ}$ ; that is, the mercury in my thermometer immersed in semifluid lead rose to  $584^{\circ}$ , and was continuing to ascend, when, owing to the shortness of the tube, I was obliged to withdraw the instrument, to prevent its bursting. I suppose therefore that  $594^{\circ}$ , as found by Dr. Irvine, cannot be materially distant from the truth. Assuming it as true, I proceeded to make several experiments on the latent heat of lead. In doing this, I was speedily led to notice, that melted lead does not by any means produce so much steam as many other metals do, when poured into water, even under the most careless management. This is the more surprising as the melting point of lead is considerably higher than that of some of these. The reason of this fact will appear from the following table:

## EXPERIMENTS

*To determine the latent Heat of Lead.*

Experiments.	Latent Heat.	Correction.	Whole latent Heat.
1	127°.8	10°.3	138°.1
	142°.8	11°.	153°.8
3	149°.9		
4	161°.8		
5	131°.5		
Mean	142°.7		145°.9

In these experiments I have by an accident, lost the notes of the weight of the vessel employed, in all but the two first instances. Consequently I have been unable to add the correction for the heat abstracted by the glass. Notwithstanding which it may be fairly inferred, that the mean latent heat of lead is about  $150^{\circ}$ , a quantity certainly unexpectedly small, and which, in many more experiments than those related, I was at pains to examine, without being able to discover any material inaccuracy. In the second experiment, where a little steam was formed, I ascertained the quantity of water lost to be four grains. If these four grains be supposed, as in the case of bismuth, to contain  $470^{\circ}$  of heat, the computed addition to the latent heat of lead will be  $20.3$ , making in all a little more than  $162^{\circ}$ . This is certainly a very peculiar and unexpected quality of this metal.

The only other substance which I have submitted to examination, for the purpose of ascertaining its caloric of fluidity, is sulphur. The melting point of sulphur is commonly stated, in elementary works, to be at  $212^{\circ}$ . But that this is not accurate, any person may convince himself, by immersing a quantity of sulphur in boiling water, where it remains altogether unaltered. By every trial, which I have been able to make, I am convinced that the fusion of sulphur takes place about  $226^{\circ}$ . I say about, because the communication of heat among the particles

of sulphur is extremely slow, and the thermometer is often incrustated with solid sulphur, which somehow or other, certainly cools below the liquid in which it is immersed. In experiments undertaken for ascertaining this point, the sulphur ought to be kept in constant motion. The results of my experiments for finding the latent heat of sulphur, are stated in the following table:

## EXPERIMENTS

*To ascertain the latent Heat of Sulphur.*

No.	Latent Heat.	Correction.	Whole latent Heat.
1	144°.56	8°.	152°.56
2	131°.	7°.	138°.
3	140°.	4°.5	140°.5
4	136°.		
Mean	137°.89	Mean	143°.68

No steam was formed in these experiments, consequently no correction of that kind is necessary as in the former instances. I have here taken the capacity of sulphur for heat at .189, from some previous trials of my own, which does not differ materially from Mr. Kirwan's determination. In the other cases I have trusted chiefly to the numbers given by Dr. Thomson, in his System of Chemistry, which I hope are correct. In every instance I have supposed the semi-liquid to have the temperature of the melting point, which I believe is generally true;

but some practice is required to seize the moment before the frozen particles float in the fluid substance. In experiments on sulphur especially, inattention to this circumstance causes very great inaccuracy, and was the cause of considerable embarrassment to me before I observed the source of my error, which I first was led to discover from the extraordinary small number of degrees which the calculation gave for the latent heat in some of my experiments where this precaution had been neglected.

I shall now proceed to give a comparative view of the caloric of fluidity of all bodies hitherto examined for the purpose of ascertaining that point.

TABLE

*Of the latent Heat of all Substances hitherto examined.*

Substance.	Melting point.	Latent heat	Latent heat in degrees measured by the capacity of water.
Ice	32°	155°.555	140°
Spermaceti	113°	145°	46°.4
Bees' wax	142°	175°	
Sulphur	226°	143°.68	27°.145
Tin	442°	500°	33°
Bismuth	476°	350°	23°.65
Lead	594°	162°	5°.604
Zinc	700°	493°	48°.3

In all these instances the latent heat is expressed in degrees measured by the capacity of the

relative solid, excepting in the cases of spermaceti and of bees' wax, which are in degrees measured by the capacity of the fluid. I endeavoured to rectify this so as to make the comparison more fair, by determining the specific heat of solid wax and spermaceti; but I have not been able to satisfy myself with either of these points, owing to the softness and consequent absorption of latent heat which a very low degree of heat induces in both these bodies. The numbers expressing their latent heats, are therefore, in all probability somewhat too low.

On inspection of the above table, there does not appear any ratio by which the quantity of caloric of fluidity seems to be guided; it certainly does not increase with the difficulty of fusion, whether we compare the number of degrees as measured by a common standard as the capacity of water, or each by the capacity of the relative solid body. The most likely conjecture is that the quantity of the latent heat has some connection with the proportion of the capacity of each substance in its solid and fluid state. If, however, this even be so, there is little probability of any advantage being derived from the knowledge of it. At least difficulties, probably forever insurmountable, will prevent the determination of the specific heat of any of the fluid metals excepting mercury.

I have thus endeavoured to add my mite to the information formerly possessed respecting the latent heat of bodies in general. The substances now ex-



mined are, those which afford the readiest and most certain means of ascertaining this point. Most of the remaining metals melt at so high a temperature; that the method of trial by water would be both dangerous and inaccurate. The same is true of most other bodies, as glass, stone, &c. A few substances of the resinous and greasy kind might indeed have been added to the list: but besides that the capacity of the latter cannot be easily determined in the solid state; the former become so extremely viscous, by the application of heat, that their diffusion amongst water would be very difficult, if not impracticable, with sufficient rapidity. There is one of the metals, however, which fuses at so low a temperature, that the embarrassments arise more from the trouble of procuring a sufficient quantity of the metal in a solid form, than from any other cause. It will be easily conjectured that I allude to mercury. I had, at one time, hopes of being able to add the latent heat of this body to the number above stated. I have been unable, however, to do this, solely from the want of an opportunity of freezing a proper quantity of that metal. I will now, however, describe briefly the manner in which such an experiment was intended to have been conducted by me, in the hope that some person, in more favourable circumstances, may be induced to put it into effect. A considerable bulk of mercury should be frozen; the larger this is the more probability would there be of an accurate result. The temperature of the apartment ought to

be as low as conveniently it can be made. The mercury ought to be enclosed in a thin glass vessel.— There must then be provided another glass vessel, with a known weight of fluid mercury : into this the solid metal must be suddenly plunged, and the new temperature noted as in the experiment upon bismuth. If all or any part of the vessel containing the solid mercury should enter into the mixture, the effects of it, in subtracting its share of the caloric, might be easily estimated. It would be difficult to ascertain the precise temperature of the solid mercury, the rapid effect of heat upon it not allowing time for such an observation. But it might be safely taken at  $-39$ , without danger of any error exceeding one or two degrees ; and consequently all the additional degrees of heat which it obtained, by mixture with the fluid mercury, are to be considered as being measured by the capacity of fluid mercury. Whence, after making the proper corrections for the heat given out or received by the air and vessels, the latent heat of mercury is expressed by the difference between the number of degrees by which the solid is raised in temperature, and the number which an equal weight of the fluid would have been depressed : the latent heat, in this case would, as in those of spermaceti and bees' wax, be in degrees measured by the capacity of the relative fluid.

In the Philosophical Transactions of London, for 1804, there is an extremely ingenious and interesting paper, by Mr. Gregory Watt, upon the regulated refrigeration of melted basalt. That gentleman,

among various observations, which do not relate to our present subject, remarks, that the large mass of basalt, which he fused in a furnace where iron was commonly melted, was said, by the workmen, to require only about one half of the quantity of fuel for its fusion than an equal weight of iron would have done. Now it seems to me not impossible to draw some conclusions from these facts, which may give us a little information regarding the latent heat of iron and basalt. In the first place, it must be supposed, that the quantities of heat produced and entering into these bodies, are to each other as the quantities of fuel consumed. That is, if a weight of fuel represented by 2, produces an elevation of temperature in the body immersed in it as  $20^{\circ}$ ; double the weight would produce double the rise. This is probably not exactly correct, but there seems reason to believe that it approaches to the truth: and therefore, that whatever quantity of caloric enters into iron during its fusion, in Mr. Watt's furnace, only one-half of that enters into an equal weight of basalt, fused under similar circumstances. This is not to be considered as applying to temperatures, which consist of degrees of various value; but to the absolute heat, according to Dr. Irvine's expression. If then a quantity of caloric, as 1, has entered into the basalt, which has been sufficient to fuse that substance, and to raise its temperature to  $90^{\circ}$  of Wedgwood, which is its melting point, and probably little or nothing higher; and a quantity of caloric, as 2, has entered into an equal weight of iron,

and proved sufficient to fuse it, and to raise its temperature to  $158^{\circ}$  of Wedgwood, at which degree it melts: how are we to proceed to discover what portion of caloric has in these instances served to raise the temperature merely, and what has entered into these bodies as latent heat? The capacity of iron for heat is by all experiments about .12; the capacity of basalt, as far as I know, has not been examined. Had I been able to procure a specimen of the Rowley Ragg, upon which Mr. Watt's experiments were made, I should have taken care to ascertain that point. But though we are not in possession of the precise number expressing the specific heat of basalt, we are not wholly without strong analogies to guide us to a probable approximation. Basalt is, by fusion, converted into a glass. Now, if any table of the capacities of bodies be inspected, it will be observed, that the capacities of the various kinds of glass do not take an extensive range, but approach nearly to one another. For the sake of illustration, I subjoin a table, affording a comparative view of the different determinations of this point:—

GLASS.	CAPACITY.	AUTHORS.
Crystal . . . .	.1929	Lavoisier
Swedish Glass .	.1923	Wilcke
Flint Glass . .	.174	Kirwan
Crown Glass .	.200	Irvine
Green Bottle Glass	.173	Author
Mean . . . .	.187	

1 to 2 ; or that twice as much heat has entered the iron as has entered the basalt : the temperatures produced, however, are only as 147 to 166, or as something more than 7 to 9. Only two-sevenths more of caloric appear to have gone to raise the temperature of the iron, than to raise that of the basalt, although double the quantity of caloric has been expended. From which it ought to follow, that the latent heat of basalt is what would remain after subtracting  $147^{\circ}$  from a quantity of caloric as one ; and the latent heat of iron is what would remain after subtracting  $166^{\circ}$  from a quantity of caloric as two. Therefore, let the quantity of heat produced by the fuel employed to fuse the basalt, be expressed by  $200^{\circ}$  : if any other number suit the fancy of the reader better, he can make the substitution for himself, and the result will be the same. Let then that quantity of heat be called  $200^{\circ}$  : it is obvious, that the heat produced by the fuel employed to melt the iron must be called the double of that number, or  $400^{\circ}$ . From the  $200^{\circ}$  subtract the  $147^{\circ}$  of temperature received by the basalt, and there remains  $63^{\circ}$ , which cannot be accounted for by the mere heating of the basalt, and must therefore be its latent heat. In the same manner, from the  $400^{\circ}$ , take the  $166^{\circ}$  of temperature received by the iron, and there remains  $234^{\circ}$  for the latent heat of iron. It is true, our present argument does not go so far as to prove that these are the actual numbers expressing the latent heat of these substances : but it renders it probable,

that the latent heat of basalt is, to that of iron, in the proportion of  $63^{\circ}$  to  $234^{\circ}$ .

If this reasoning be admitted as correct, it might be possible to determine, by experiment, the proportions which the latent heats of various other bodies bear to each other. All that would be necessary for this purpose would be to know the melting points of these substances, the quantity of fuel which was necessary to convert them into fluids, together with their specific heats. These things being ascertained, we might proceed somewhat in the manner of which the above is a specimen. By operating on very large quantities of the body examined, the chances of inaccuracy would be materially diminished; and though in this way, it is clear that we never could learn the precise quantity of the latent heat of any body, but only the ratio in which one number stands to another; yet, it is not impossible, that means might be found to connect this chain of ratios with another chain, of which not only the ratios, but the quantities were known. This might be done if the latent heat of any one body, of which the ratio could be determined by the method just stated, could be found precisely, either by its commixture with water, or with any other substance, such as mercury, which might be more convenient. The latent heat of the rest might then be discovered by a simple rule of arithmetic.

It is probable that the calorimeter might be applied to the determination of latent heats, in cases where



the ordinary plan could not be employed : at least, it seems possible to enclose in a vessel, of which the specific heat is known, a determined quantity of any body in a state of fusion. It is unnecessary to describe any further the method of proceeding in this case, which must be obvious to any person conversant on these topics. Perhaps the objections against the accuracy of the calorimeter would not be so forcible, in such circumstances, as they have been supposed to be in many other cases.

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## ESSAY II.

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### *ON THE AFFECTIONS OF SULPHUR WITH CALORIC.*

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**S**ULPHUR has been an object of attention to the chemists from a very early period. They have examined it in various ways, and subjected it to the ordeal of innumerable experiments, by means of which they have succeeded in making many discoveries regarding its qualities, not less useful in the science of chemistry, than in the practice of the arts. In one respect, however, less progress has been made than might have been expected. The affections of sulphur, by caloric, have not been so much attended to, nor have experiments on the operation of heat upon it been so numerous or frequent as the importance and familiarity of the object seem to have demanded. If we except Mr. Kirwan's determination of the specific heat of this body, scarcely any

other observation has been made at once with the merit of accuracy and utility. I do not mean to say that no observations at all have been made, or that those have been wholly destitute of claims to praise; but that less has been done in this way than on many other subjects, and that room is still left for additional remarks.

Sulphur, at the ordinary temperature of the atmosphere, is always a solid, brittle, hard body, which expands rapidly and unequally when heated, owing to its being an extremely indifferent conductor of caloric; so that the external parts expand more quickly than the internal, and are thus forced to a separation. Cold, as far as is known, produces no remarkable effect upon this body. When subjected to friction it accumulates on its surface a large quantity of electric matter; consequently it is a non-conductor of that fluid, since it is impossible to collect electricity on a body that freely disperses it in every direction. The differences between solid and fluid sulphur in their relations with the electric fluid, are not well understood. A spark can certainly, as I have found, be drawn through fluid sulphur, which is then thrown into violent agitation: but it may be doubted whether it passes through the substance of the sulphur or merely forces the particles out of the way: the subject is worthy of further investigation. The non-conducting power of ice, contrasted with the opposite quality of water, is extremely remarkable in a chemical point of view.

The specific heat of sulphur, according to my experiments, is .189, which does not differ much from Mr. Kirwan's determination, and may be considered as confirming the accuracy of his observation. The melting point of this body I have found rather higher than it is usually estimated, and to be at  $226^{\circ}$  nearly. In making this experiment, the thermometer is apt to be encrusted with solid sulphur, in consequence of which the mercury falls lower than it ought to do, but that may be prevented simply by keeping the thermometer in constant motion.

Sulphur, in all probability, changes its capacity for heat suddenly during fusion: at least it remains hard nearly to the very last, and flows into an extremely thin liquid. It may be frozen and melted repeatedly, without any alteration in its properties. If the application of heat be continued for some time, a very singular change takes place in the appearance of the sulphur: instead, like other bodies, of continuing to become thinner and thinner by the increase of temperature, and at last being converted into an aëriform fluid, this substance grows very thick and tenacious; and though its solidity cannot be restored in this manner, the cohesion between its particles is very greatly augmented. It is asserted that no chemical alteration of the body is effected by this process: no oxygen, or very little, is said to have been absorbed; and the sulphur, after undergoing the thickening, produces nearly the same quantity of sulphuric acid as before. Its colour,

an expansion. Water also, from  $32^{\circ}$ , Fahrenheit, to  $40^{\circ}$ , has been asserted to contract as its temperature rises. There has been, especially of late, some dispute upon this subject, which has become of more importance since it has interfered in the evidence of Count Rumford's ingenious opinion regarding the transmission of heat through fluids. It seems probable that we should have some chance of deciding the fact of the expansion or contraction of water immediately after fusion, by examining its specific gravity. I propose to try this as soon as I shall have a proper opportunity and apparatus. In this manner, at least, very small variations of density may be detected.

It became an object of some curiosity, to learn whether this thickening of sulphur is accompanied by an expansion or contraction of its volume. I examined this point by filling an ounce phial completely to the very brim with melted sulphur: I then applied heat till the temperature was about  $400^{\circ}$ , and the sulphur very thick and tenacious, and emitting a little vapour. The process of cooling was then carefully observed, and it was remarked that a steady contraction of the fluid ensued till it arrived at the temperature of  $226^{\circ}$ , at which point it began to expand, and thrust upwards a large nipple-like projection. The sum of the contraction, from  $400^{\circ}$  to  $226^{\circ}$ , amounted, as I computed, to an eighteenth part of the volume of the sulphur at  $400^{\circ}$ , and to a seventeenth part of its volume when fluid at  $226^{\circ}$ .—



The expansion, during freezing, may less accurately be taken at one-fortieth of the mass of fluid sulphur. These things being admitted, it ought to follow that a piece of solid sulphur, at  $226^{\circ}$ , should swim in fluid sulphur at the same point: but this does not happen if the temperature of the solid is considerably below the point of fusion. This may be accounted for by supposing, what is otherwise extremely probable, that solid sulphur expands rapidly by heat, and consequently contracts equally rapidly by cold: so that at a certain number of degrees below  $226^{\circ}$ , its specific gravity becomes equal to that of its corresponding fluid, and at every degree of heat lower, than that it must inevitably sink till its temperature is duly raised. Some pieces of cold sulphur, which I threw into the same substance, melted, sunk gradually, and were almost immediately fused, that is to say, the fusion took place at the surface before the sulphur, which is a very bad conductor of heat, could receive enough to be expanded, so as to rise in the fluid.

It was formerly observed by my father, that melted spermaceti and wax cooled in such a manner as to render it probable that they parted with their latent heat gradually. I thought it worth while to try in the case of melted and thickened sulphur, whether any thing could be remarked that should indicate a change of the capacity of that body for heat. With this view I made several experiments, of which here follow the results.

## EXPERIMENTS

*On the Cooling of melted Sulphur.*

Experim. I.		II.		III.		IV.	
Temp.	Diff.	T.	Diff.	T.	Diff.	T.	Diff.
530	0	550	0	496	0	470	0
511	19	530	20	84	12	62	8
484	27	507	23	73	11	48	14
457	27	486	21	60	13	34	14
436	21	466	20	47	13	21	13
416	20	448	18	35	12	408	13
398	18	432	16	22	13	397	11
386	12	417	15	10	12	83	14
374	12	403	14	398	12	71	12
364	10	390	13	386	12	360	11
357	7	376	14	374	12	346	14
350	7	364	12	362	12	330	16
343	7	350	14	351	11	314	16
333	10	336	14	340	11	302	12
323	10	318	18	328	12	291	11
312	11	303	15	315	13	284	7
303	9	293	10	305	10	274	10
294	9	284	9	296	9	269	5
285	9	276	8	286	10	262+	7
278	7	269	7	275	11	256	6
271	7	264	5	267	8	249	7
264	7	257	7	262	5	243	6
257	7	250	7	57	5	237	6
251	6	244	6	50	7	231	6
245	6	238	6	45	5	226	5 freezing
239	6	232	6	39	6		
		226	6	33	6		
				27	6		
				226	1		

The quantity of sulphur employed in these experiments varied considerably in the different instances from two ounces to six. There evidently appear considerable irregularities in the progress of cooling, which must be attributed to currents of air impossible to be prevented at such high temperatures. In such cases it is very difficult to draw unobjectionable conclusions. But from an inspection of the above table, it appears that some retardation in the progress of refrigeration took place about the temperature of  $350^{\circ}$ , a little above, and a little below, from which it ought to follow, that near that point some enlargement of the specific heat must have occurred, since no other cause would account for the retardation at the same point, in all the experiments related.

The thickening of sulphur by the application of caloric, is curious in another point of view, when we consider the process by which bodies are converted from the solid to the fluid state, and from that to the vaporous, as well the general expanding, and liquefying property of this principle. It appears pretty evidently that a solid body differs chiefly from its corresponding fluid in the difficulty or impossibility of moving its particles upon each other. The particles of the fluid, on the contrary, move over each other with the utmost freedom. Now to convert a solid as ice or sulphur into the liquid form, it might seem likely, at first sight, that the

caloric, entering the solid, by its expansive and repelling powers, destroyed the cohesion between the particles, and permitted them to slide smoothly and easily over each other, or, in other words, made a fluid of it. But though this explanation does not lie open to many objections in the case of those bodies which expand during fusion, that is not the case where a contraction takes place during that process, as happens in the instances of ice and sulphur. It is clear that caloric cannot produce liquefaction in these substances solely by destroying cohesion, since, as far as we know, cohesion increases as the distances diminish, in a ratio not ascertainable. Therefore no contraction of any body can possibly cause its particles to have less attraction for each other. Of consequence, ice, which contracts during fusion, could never be melted by heat, if heat produced no other effect on it than to diminish the cohesion of its particles.

In order to solve this difficulty, recourse has been had to another principle, better adapted for the purpose. It has been said that the particles of ice have a certain degree of polarity, a disposition to assume a peculiar situation, in fact that they have a specific power of adhering to each other by their ends or otherwise, and this in allusion to the well-known and striking facts of magnetism, has been called their polarity. It cannot, however, be like the magnetic polarity, as far as we know, in all respects. It is not certain,

nor even probable, that these particles arrange themselves in any way relative to the position of the poles of the earth. All that is meant to be conveyed by this expression is, that the particles of solid bodies do not simply adhere, but adhere with a much greater force at one point than at any other. Caloric, therefore, by destroying this polarity, may effect a compression of the bulk of a fusing body, the particles no longer refusing to occupy the interstitial spaces. All solid substances however, do not undergo the same process when exposed to the action of heat. Many of these, it is well known, do not contract when melted, and, consequently, either have no polarity of particles to be destroyed, or their polarity is such that the particles adhere together precisely in the manner in which they occupy the least room. We are very far from understanding what is the nature of this polarity, whether all bodies possess it, what relation it has to the general properties of matter, or what to the peculiar phenomena of magnetism. These inquiries are, however, not only very curious in their own nature, but are likely to throw considerable light on the operations of chemistry when they shall receive a satisfactory investigation.

The phenomena of melted sulphur which led to these observations are also to be considered as shewing the existence of this quality, which is named polarity, in a situation where it has not before been remarked. If we are right in concluding the thick-



ening of sulphur to be purely an effect of caloric, and an effect unaccompanied by any contraction of bulk, but, on the contrary, by an expansion, it must be ranked in the same place with the freezing of this body which, as we have shewn, is also accompanied by an expansion, and a polarity of particles. Now surely it is a very surprising thing that at the melting point,  $226^{\circ}$ , caloric should have the power of destroying the polarity which causes the solidity of sulphur, but that the addition of ninety-four degrees of caloric, so far from confirming the fluidity, should actually appear to restore the polarity of the particles of this body, at least in a certain degree. It appears very difficult to reconcile these phenomena with each other. The caloric acts apparently in two manners directly contrary, and if we allow the polarity of solidity to be destroyed by heat, it will be hard to explain how the polarity of incrassation should arise from the same cause. I do not pretend, at present, to be able to give a satisfactory elucidation of this difficulty, which seems to me to be very little consistent with some parts of the most generally received theories of caloric. The most natural inference that occurs to me is, either that both sorts of polarity have their origin from another cause than caloric, or at least that in one of the instances another agent may be suspected. We are exposed to many difficulties in arguments of so complex a kind as the present, and conjectures ought not to be admitted without very solid grounds. But it is scarcely pos-



sible to imagine the interference of any other influence in the destruction or suspension of polarity, excepting that of the electrical matter. But besides this, there are at any rate, considerable reasons for believing that electricity actually exists as a component part of many and probably of all bodies. The quantities of it in different bodies cannot be doubted to vary, and since we well know that the conducting powers of substances differ from each other, so in all likelihood do their capacities or specific electricities. The phenomena of galvanism support the same opinions. If then bodies contain the electric fluid as one of their component parts, it is not to be supposed that they will pass from the solid to the fluid state without some effect being produced upon their electricity. In the case of ice, we know that ice is a non-conductor, but that water is a tolerable conductor. Sulphur also is a non-conductor, and liquid sulphur conducts ill; it does not, however, wholly refuse a passage to the electric spark, though the difficulty is considerable. Ice therefore, it is not improbable, receives during its fusion a quantity of electricity, which may destroy the polarity of its particles and contribute to the formation of water, and in the same manner sulphur during its fusion. If this were a just account of the matter, either of these bodies being raised to a high temperature, and having at the same time their capacity for electricity diminished, would immediately become thicker, the antipolar influence being in part removed; this we

see happens in the instance of sulphur. I do not mean to offer these remarks as any more than conjectures of which the truth is very uncertain, and very difficulty referable to experiment.

The capacities of bodies for electricity cannot be exactly found, for want of an accurate electrometer, which should be as good a measure of electricity as the thermometer is of heat. But something might be done in this way; by having a standard Leyden phial charged, so as to produce a certain effect on the electrometer, and discharging it into another phial filled with the body of which the capacity is required, some idea might be obtained of the quantity of electricity necessary to abstract from the charged phial to establish an equilibrium with the other. And this quantity, which would, probably vary according to the body examined, would, if equal weights or bulks were used, give at least the order in which substances should be arranged; many precautions would of course be required to obtain even a small degree of accuracy of result in such experiments. One observation, however, has been made upon metals fused by electricity. They do not undergo real fusion, but the polarity of their particles is destroyed without the necessary quantity of heat being present to retain them in fusion, and the whole is converted into a powdery mass, where probably some particles are disengaged and some imperfectly joined to those nearest to them. I was unable to fuse ice in this manner, but a piece of that body

held to the conductor of an electrical machine in motion seemed to melt faster than it otherwise would have done.

If this view of the action of the electric matter in destroying the polarity of the particles of solid bodies were well founded, some consequences would follow which might be readily submitted to the test of experiment. Water, as has been formerly observed in the first part of these Essays, can be cooled below its ordinary freezing point for a number of degrees without losing its fluid form. The reason of this is far from being well understood; and much less can any account be given of the sudden consolidation of water so cooled, by being touched or slightly agitated. The same things are true of the solutions of salts which crystallize by similar treatment. If the electric fluid interfere in the manner we have supposed in the process of fusion, the reverse of that operation ought to occur in that of freezing. That is, a quantity of electricity ought to be discharged. This, however, may be supposed to require the help of some conducting body to carry it off readily. But if water be enclosed in glass, or at most have contact with no other substance but one stratum of the atmospheric air, this facility may not be afforded, and the water may still remain fluid. Agitation, however, or the touch of any solid substance may remove the obstruction and convert the water into ice. There must be some limit, however, to this process, and water probably cannot be retained in a

state of fluidity even in the most perfect state of rest, below some determinate point. But if electricity have any share in the production of fluidity, it is not unlikely that water strongly electrified might be cooled considerably lower than it has hitherto been. Unknown circumstances, however, might undoubtedly conspire to prevent this effect. It might be worth while also to learn whether water contained in a metal vessel communicating with the earth could be cooled equally with the same substance in a glass vessel. Some of these points I intend to take the opportunity of the ensuing winter to examine when the state of the weather shall be convenient for such experiments.

Since writing these observations I have learned that Mr. Cavallo, in his Treatise on Electricity, has some remarks on the electrical properties of bodies which are applicable in the present argument. It has, I find, been already determined by experiments long ago made, that common rosin which when solid is an electric, becomes after fusion a conductor of electricity. It will be recollected that this was one of the bodies which were placed by me on the conductor of a large machine and through which sparks were drawn. It affords me pleasure to observe that my experiments coincide with those of more experienced electricians, and I am very certain that the other substances, sulphur, spermaceti, and wax, the conducting powers of which were examined in their fluid state, were all of them at least

equal in this respect to the rosin, and the sulphur especially considerably superior. Whether the account here suggested of the method in which fusion is effected has any foundation or not is certainly very doubtful. I am far from attributing to it any considerable degree of probability, and am sensible that it can as yet only claim admission into the list of conjectures. Arguments on topics of so abstruse a nature are always conducted with difficulty, and never without danger of error. But however this point may be determined, I can revert with more confidence to my original proposition, that in the processes of the fusion and incrustation of sulphur some other agent than caloric is most probably engaged. And *this is* equally true, though we may be *unable* to detect the presence or explain the properties of that agent, though our speculations upon the interference of the electrical fluid may prove destitute of foundation, and though we may be compelled to imagine some undiscovered property of matter to act this part, to perform which caloric alone is not adequate.

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